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ORIGINAL ARTICLES.

CAN THE CÆSAREAN SECTION BE SAFELY SUBSTITUTED FOR CRANIOTOMY IN THE UNITED STATES AT THE PRESENT TIME?

Read before the Medical Society of the District of Columbia, January 12, 1887.

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Much interest has been excited of late in the improvements which have been made in the ancient and time-honored operation of the Cæsarean section. To Dr. Harris, of Philadelphia, are we mostly indebted for this revival of interest. He has repeatedly pointed out that this operation, which, in the past, has been regarded as almost universally fatal to the mother, can now be performed with great safety, and with the result of saving more lives, when resorted to early, than by any of its alternatives. This statement is made by Dr. Harris after collecting the histories of a comparatively large number of cases in this country and in Europe. These remarkable results have been achieved by a few men, mostly in Germany, who have had a large experience in abdominal surgery, and who brought to their assistance the many improvements which a number of operators had been gradually making, in different parts of the world, in the Cæsarean section for many years. These successful operations have aroused the enthusiasm of a few writers to such a degree that they have been led to declare, with great fervor, and an eloquent display of statistics, that craniotomy upon the living fœtus would hereafter be unjustifiable, and that the improved Cæsarean section must not only be the operation of election in the future, but of necessity also. Thus Dr. Meadows, of London, within the past three months, in a paper read before the British Gynecological Society, upon "The Total Abolition of Craniotomy," was so carried away with the wonderful and surprising figures of Dr. Harris, that he exclaimed, in a sudden burst of enthusiasm: "Where, I ask, is craniotomy now?" Dr. Meadows quite convinced himself, but, according to the record, none of his audience, that craniotomy should be forever abolished from the list of justifiable operations. It was generally agreed, in the discussion which followed, that the best thing ought to be done; that whenever the Cæsarean section offered the best chance it should, of course, be urged upon the family, but that there would always remain a certain small proportion of cases, mostly in primiparæ, where craniotomy would have to be performed.

When this subject was discussed in another society, in our city, two years ago, I took the ground strongly that past experience, as well as the teaching of nearly all the text-books in use by the colleges, declared that craniotomy, when indicated, should be considered the operation of election, and the Cæsarean section the operation of necessity.

In view of the growing success of "the improved Cæsarean section" abroad, and of my growing interest in abdominal surgery, I am inclined to change my views, exactly reversing the position stated above, making the Cæsarean section the operation of election, when possible. This, of course, leaves craniotomy as the operation of necessity; and when necessary, therefore, I would feel compelled to resort to it. I cannot agree with those writers who would entirely abolish it, and who denounce craniotomy as "child murder," as an "abominable crime," as that "murderous operation," as "killing the infant," as the "deliberate and cold-blooded murder of an unoffending child," etc. I think craniotomy, even upon the live child, *may become* a perfectly justifiable operation under certain exceptional circumstances.

Science advances, and as new facts come to our attention, and as the success of abdominal surgery strides on with such surprising and startling rapidity, we are entitled, without being considered inconsistent, to change our views, and to be convinced, by the onward march of events, that new practice, based upon late experience and improved statistics, may be better and safer than the old.

While I should be slow, as a teacher of obstetrics, to allow the writers in the ephemeral and frothy medical journals of the day to offset, with a few newly-acquired statistics, the crystallized teachings of the regularly authorized and recommended text-books, new as well as old, for the guidance of students and practitioners of medicine, I should hail with infinite satisfaction any practice which would give to the mother a better chance for life than craniotomy offers, and at the same time, while not diminishing her chances, afford us an opportunity of saving the child also. I do not think it is sound practice to be governed by the mere statistical intent to save the greatest number of lives without any reference to the value of those lives. The Cæsarean sectionists use an argument which sounds well, and which, for statis-

tical purposes, would undoubtedly carry a point, and perhaps an audience; inasmuch as by the performance of the Cæsarean section more lives could possibly be saved for the census to record among the inhabitants of our country than as if craniotomy were sometimes done. Of course, 50 per cent. of the "lives involved" are lost, as well as a very small per cent. of the mothers, but I thought, when I heard an argument some time ago upon this subject, that the speaker rather strained a point, when he added the 50 per cent. of the already dead or sacrificed fœtuses to the ascertained mortality to the mothers, of the craniotomy operation. I doubt if every one would stop to think about the unborn children, when the mortality of craniotomy would be stated, upon this basis of calculation, to be 50 per cent. higher than the deaths of the mothers would properly place it. Thus, of 100 craniotomies, if the mortality should be stated to be 60 per cent, the general reader might think that sixty of the mothers had perished as a result of the operation; when, according to this mode of making statistics, the statement would mean the death of fifty fœtuses and only ten mothers.

We cannot, as conscientious physicians, be bound in our practice by cast-iron rules, whether they are medical, ethical or theological. As long as the good Lord continues to make human beings to differ so widely from each other, just so long shall we be compelled to judge each case by its own peculiar symptoms, environment, vital force, extent of deformity or obstruction; and be governed to some degree by the wishes of the patient who engages our services.

This is a free country and we, of course, have the inalienable right to refuse to remain responsible where our advice is rejected. But the patient has the same right to refuse to have an operation done which gives her so little chance of recovery as even the improved Cæsarean section does in this country.

If we are in attendance upon a case of midwifery which lasts longer than twenty-four hours, where the powers of the patient are showing positive signs of failure, where the forceps have been tried a number of times and failed, where the head is too far down to resort to version, and where, in order to save the now rapidly sinking mother, a corps of consulting physicians submit to the agonized husband and wife the sad alternative of craniotomy or some modification of the Cæsarean section, and they refuse to permit a cutting operation to be done—what is the attending physician to do? Suppose he has bound himself by some cast-iron rule, or allowed somebody else to bind his hand and his conscience for him, never, under any circumstances, to destroy what little life is left in the long compressed and nearly lifeless unborn fœtus—is this woman (who has engaged this physician to see her safely through her labor) to be left in jeopardy while the fœtus is given the precious hours, to become unmistakably dead, which may decide adversely the fate of the mother also! Those who would refuse to do craniotomy upon the live child, but would be willing to do it if the child were surely dead, will sometimes, while obeying their cast-iron rule, let the mother slip through their fingers also. I should hold with Barnes upon this point, who says, on page 845 of his Obstetric Medicine and Surgery:[1] "On the continent especially, it is still urged by some to wait until the child is dead. If it be admitted—and the conditions of the case involve these postulates—1, that the child cannot come through alive; 2, that the operation is undertaken in order to save the mother, waiting till the child is dead is opposed alike to reason and to humanity. It seems a refinement of casuistry to distinguish between directly destroying the child, and leaving it exposed to circumstances which must inevitably destroy it, and it is risking the very object of our art, to wait for the lingering death of the child until the mother's life is also imperilled."

In speaking of the Cæsarean section, on page 855 same work, Barnes says: "If the operation could be done at a chosen moment, and so improved as greatly to increase the probality of saving the mother, then the already high probability of rescuing the child might turn the scale in favor of the Cæsarean section and against craniotomy. Unfortunately, art has not yet reached this point. The mortality to mothers from the Cæsarean section is still so great, whilst that from embryotomy, in fitting cases, is so small, that we are unable at present to raise the Cæsarean section to the rank of an elective operation."

In view, however, of the success of the improved Cæsarean section, I should be inclined to hold with those who would, if they could, raise the Cæsarean section to the rank of an elective operation; but we should hold ourselves free, in case this elective operation were not elected by those who had the right to vote, to perform craniotomy, upon the live child even, if we believed that by doing so we could save the mother. I should agree with Lusk, 1885 [Science and Art of Midwifery, p. 425] that "If in any case the decision is left to the physician, he should regard the welfare of the mother as of paramount importance. . . . The duty of the physician is, however, to his patient. He is not to constitute either judge or executioner."

As the work of Prof. Parvin has been published within the past month, it may be of interest to record his views as the latest authoritative statement from an American stand-point. Professor Parvin says, upon page 650 of his book:[2] "Some, indeed, have had so strong a repugnance to directly sacrificing the life of the child, that they have done it indirectly, waiting until it died before resorting to the operation; thereby in no sense evading the responsibility for its death, and, at the same time, this delay has added to the perils of the mother.

"The principle of morals upon which most obstetricians rest the right to sacrifice the child for the sake of the mother is a very old one, and has met with general acceptance. That principle, clearly enunciated by Cicero, for example, and sustained, in general, by moralists of all ages, is, that if two lives are in such peril that both cannot be saved, but one will be, by the sacrifice of the other, let that life which is of the least value to the State or to society perish. It is unnecessary to show that the adult woman, with her various domestic and social duties, has a life of

[1] Published in 1885 [only last year].
[2] Science and Art of Obstetrics.

greater value than that of the unborn child, and therefore, while the duty of the obstetrician is to save both when he can, if either is to be sacrificed, let it be that of the latter—in other words, if, in a given case, embryotomy is a less risk to the mother than Cæsarean section, the former should be selected. This is a rule of obstetric ethics which cannot be set aside."

There is an element of unfairness in comparing the statistics of craniotomy as given by Tyler Smith in his book, written as far back as 1858, and by Churchill about the same time, with the improved statistics of 1884–5–6 of the Cæsarean section, done under all the detail of the antiseptic methods, and in the light of all the wonderful improvements in abdominal surgery.

It is my belief that more *mothers* would be saved, and would be put to much less pain and in much less danger, by the "timely" performance of craniotomy, done under all the antiseptic precautions, than by the "timely" performance of the improved Cæsarean section, if we judge by recent reports of the mortality attending this operation in the United States. It is my belief, also, that more *lives* would be saved, if we include those of the unborn fœtuses, by the timely performance of the improved Cæsarean section than by craniotomy, and as the Cæsarean section is a much more conservative and clean surgical procedure than the "horrid and detestable operation of craniotomy," as it is now called by some, I should, with my knowledge of abdominal surgery, *greatly prefer to* do it. One of the chief points I am arguing against is, that it is cruel, unscientific, and impracticable to be bound by any inflexible rule in the management of these unfortunate patients. To "utterly abolish craniotomy," as recommended by Meadows and a few others, would leave us with our hands tied in some cases, and subject us to the mortification of being superseded by a physician who was governed more by the circumstances and necessities of this particular case, than by an unscientific prejudice. Our minds and hands should be free. Cases differ; people differ; obstructions differ; vital force, and the dispositions of patients, so differ that, as conservative physicians, we should act as the requirements of each case are presented to us. A timely and successful Cæsarean section, done before the patient had been exhausted by long-continued, various and unsuccessful efforts at delivery, cannot be fairly compared and quoted against an unsuccessful craniotomy, performed upon a woman who had been in labor several days, who had the forceps tried a dozen times by half as many doctors, who had completely exhausted themselves in their fruitless efforts to pull out the child, and finally, when they are worn out, in the middle of the night, perhaps, and by the aid of a tallow candle, with imperfect instruments, poor assistants and no antiseptics, they do a bungling craniotomy, through parts swollen, dry, and ready to lacerate or inflame.

It is no argument, in favor of always doing the Cæsarean section, because a woman suceeeds in doing it upon herself with a carving knife, and closes the abdominal wound with sticking plaster. Neither is it just the thing to compare the marvelous results of Leopold and Sänger, in saving seventeen out of nineteen mothers, and all the children, by the improved Cæsarean section, when they bring to bear all their skill in abdominal surgery, have a corps of trained assistants and nurses scarcely less able than themselves, with craniotomy done under the circumstances just named, by inexperienced operators, and under the worst possible circumstances for success. Such comparisons are odious as well as incorrect. In the practice of medicine or surgery, it is very difficult to make correct comparisons, the cases are so dissimilar.

In Germany physicians can control the circumstances of their operations, and their patients, better than we do in this country. They have better and more opportunities to perform timely Cæsarean sections than we do in the United States. Having more cases of pelvic deformity than occur in America, they have made more thorough and correct studies in pelvimetry than we have, and make their diagnosis and perform their operations earlier than we do, and consequently, I believe, save more of their patients.

An early diagnosis, and the consent of the patient and her friends, to an early Cæsarean section, is more than half the battle. One reason, I believe, for the unwillingness of these unfortunate women, and those directly interested in them, to have this operation done early, is the belief of their physicians, as well as their own fear, that the result will be fatal; and they delay, and resort to other and unavailable means, until they have frittered away golden moments which cannot be regained.

Dr. Harris reports that there has been done, in this country, 144 Cæsarean sections, saving fifty-four or 37½ per cent. of the mothers; (and emphasis seems to be laid upon the point that sixty-four children were living when delivered. Perhaps a dozen or more of those children were dead in less than a week, and perhaps only twenty or ten lived to grow up.) According to the same authority, the first fifty Cæsarean sections done in the United States saved 54 per cent of the women. The last fifty Cæsarean sections done in the United States has saved but 24 per cent., a progress in the wrong direction of over 50 per cent. Dr. Harris adds to this sickening statement the information that, "operations performed in good season, when the condition of the woman was favorable, have saved 75 per cent. of the cases in this country, and 80 per cent. of the children."

These facts must have been culled from a few isolated cases, inasmuch as the last statement of operations done in the last decade, and seven months added to it, "ending August 1, 1886, there were thirty-seven Cæsarean sections—saving only eight women, or 21 23-37 per cent." Thus, with all the improvements in antiseptic abdominal surgery, in the last decade of the 19th century, in the United States, nearly 84 per cent. of the women operated on died. So that, in our country, instead of the statistics improving, they have been steadily growing worse and worse, as shown in the following late statistics and letter of Dr. Harris:

September 17, 1886.

Cæsarean operations of the United States	144
Women saved, 37½ per cent.	54
Children living when delivered	64
First 50 operations, saved 54 per cent.	27
Last 50 operations, saved 24 per cent.	12
Operations for decade ending Dec. 31, 1855	25
Women saved, 48 per cent.	12
Children living	13
Operations for decade ending Dec. 31, 1865	24
Women saved, 45 5-6 per cent.	11
Children living	10
Operations for decade ending Dec. 31, 1875	36
Women saved, 27 7-9 per cent.	10
Children living	11
Operations for 10 7-12 years, ending Aug. 1, 1886	37
Women saved, 21 23-37 per cent.	8
Children living	16
Late operations, nearly 84 per cent. of this division	31

Operations performed in good season, when the condition of the woman was favorable, have saved 75 per cent. of the cases in this country, and 80 per cent. of the children.

It will be seen by this record that the number of operations is gradually upon the increase, and that the results are steadily becoming worse year by year. If we take the last five years—August 1, 1881, to August 1, 1886—we have nineteen operations, ending fatally in seventeen cases, saving but 10 10 19 per cent. Of the children, fourteen were also lost; of which two were destroyed by craniotomy, and one had its skull fractured by the forceps. In this list is not included an operation upon a moribund woman, performed in the interest of the fœtus.

This is a frightful picture! In fact, it makes one sick at heart, in view of the fact that Europe, by the Sänger method, has saved 78 per cent., or 26 women out of 33; and 31 children.[1] There has been very bad management somewhere, and I am inclined to believe that much lies in the fact that American obstetricians have not paid sufficient attention to pelvimetry, and are wanting in the skill that has been acquired in the maternities of the old world, by which they first determine the possibilities of delivery *per vias naturales*, and finding the measure of obstruction, resort promptly to the knife without endangering the life of the woman by intermeddling and useless delay. ROBERT P. HARRIS.

Great stress has been laid by those who would always do the Cæsarian section, and never consider craniotomy justifiable if the child was not known to be dead, upon the recent successes in the Cæsarean section by the "Sänger method" in Germany. But Germany, unfortunately for this argument, is not the United States. To quote from recent authority in the October number of the *American Journal of Obstetrics*, page 1021, by Garrigues, who claims that the Sänger method should as properly be called by his name as by Sänger's, and further, that there is really nothing in this "method" after all, as the most successful operators get on better without using it—Garrigues says: "It is of particular interest to see that one operator, Prof. Leopold, of Dresden, has operated nine times, saving eight women and all the children. The two maternity hospitals, of Dresden and Leipzig together, have had sixteen operations with fifteen maternal recoveries, and the survival of all the children." This, I am glad to admit, is a most successful and glorious record, and one which American operators should strive to equal. But it is not fair or correct to state this phenomenal success as the present standing of the improved Cæsarean operation. You might as well quote the phenomenal success of Mr. Tait, in doing 146 ovariotomies without a death, as the present happy standing of ovariotomy throughout the world, and expect others to obtain the same results.

Garrigues evidently had this same thought in his mind when he wrote, less than a month ago, page 1021—"It is not to be expected that this record will be kept up to its present standard. So far, the excellent results are *due to the fact that* so large a proportion of the cases have been operated on by one man, and a still larger proportion by a few men, all intimately connected, all perfectly familiar with antiseptic precautions, and skilful gynecologists. It is to be expected that when the operation becomes so popular that it is performed by many, and less well prepared operators, the results will again decline proportionately."

Garrigues says, "further, on the other hand, I am not prepared, with several authors, to teach that the improved Cæsarean section should be substituted for craniotomy, and to stamp, as an abominable crime, the destruction of the living fœtus, if by such a sacrifice, there is reasonable hope of a safe delivery for the mother. We must remember that similar antiseptic precautions to those upon which success in the new operation seems *exclusively to turn*, have benefited the operation of craniotomy."

Garrigues then refers to three recent craniotomies done by himself, two upon the living fœtus. All the mothers made an excellent recovery, and at no time presented any serious symptoms—one of them did not even have the slightest fever. In view of the recent successes in the Poro operation, I should feel disposed, in cases where there was a demand for the Cæsarean section, and the parties interested consented to have it done, to go still farther, and remove the uterus, or at least the ovaries and tubes, and thus make it forever impossible for the woman to be environed by the same dangers again.

It has been demonstrated that the success of repeated Cæsarean sections on the same person is greater than first operations, but is nevertheless, a dangerous procedure. I am convinced that the failure of the Cæsarean section to succeed in our country is largely owing to the fact that the operation is performed after the patient has been worn out by prolonged and useless efforts to deliver her. If physicians and patients only believed this operation was the best one to perform and that it should be done early, and that it did not mean sure death, as so many seem to think, this obstacle would be removed and the door opened to a more successful future. Until we can do better than to lose 29 out of the last 37 Cæsarean sections, in the United States, we cannot bind ourselves never to do craniotomy, even upon the living child. According to Harris, of the last nineteen op-

[1] Medical News, Philadelphia, Sept. 18, 1886, p. 317.

arations, (C. S.), seventeen of the mothers died. These nineteen Cæsarean sections were all done within the past five years, and fourteen of the children were lost, a not very brilliant show for American operators. Garrigues saved more mothers than this by doing his three cases of craniotomy. It cannot be claimed that the results would have been improved by the practice of the Sänger method, inasmuch as there have been five Sänger Cæsarean sections done within as many years in the United States, and every one of the women died promptly.

There is something radically wrong, I fear, in our teaching as well as in our practice. If we could control the circumstances and environment of our patients as surgeons do in Germany, I believe we could succeed as well as they do; but physicians as well as people will require a good deal of educating, evidently, before such a happy state of things will be reached in America. While therefore, I should greatly prefer to do the Cæsarean section and should consider it, if called in time, the operation of election, I cannot see that we have reached a position in this country, which would give us the right to "totally abolish craniotomy," and substitute for it at present, an operation which I have shown is so seldom successful as even the "improved Cæsarean section."

I subjoin as an appendix to my paper (with the kind permission of the author, Dr. Wm. H. Parish, of Philadelphia, whose entire article will appear in the forthcoming volume of the Transactions of the American Gyecological Society) extracts from that portion of his paper relating to the Cæsarean section in the United States, and also remarks of Dr. Robert Barnes on Dr. Meadow's monograph referred to above, and which I adopt as a part of my paper.

APPENDIX.

Let us now turn to the mortality attending all Cæsarean operations in the United States. Of 144 operations only fifty-four or 37½ per cent. of the mothers have recovered. Ninety mothers died out of 144 operated on, a truly frightful mortality.

But many of these operations were performed before abdominal surgery had attained its present high degree of success. Has the Cæsarean operation profited by the conceded recent surgical advances? Let us see: Of the fifty operations first performed in the United States, twenty-seven of the mothers, or 54 per cent. recovered; and of the last fifty operations, only twelve of the mothers—*i. e.*, only 24 per cent. recovered. The mortality has steadily increased during recent years, until of thirty-seven women last operated upon, twenty-nine have died; and of the last nineteen operations, seventeen of the mothers have died—a horrible mortality! In these nineteen last cases, all occurring during the last five years, there were fourteen dead children. In the last nineteen Cæsarean sections, then, there were only two mothers saved, and only five children survived. Should not American surgeons and obstetricians hang their heads in shame at such a showing? Is it not time that we should look into the reasons for such shameful fatality following this operation in this country? The results in Europe show with certainty that such terrific mortality is not inseparable from the operation, when rightly done under proper conditions.

The two prime factors in producing such fatality are, doubtless, delay in performing the operation, and the attempts made at other methods of delivery before making the section. Look at the number of children dead, when the operation began! The death of the child could not have been due to the operation, but must have been produced prior to its performance. Of the 144 operations, eighty children were dead. What caused their death? Quite certainly, either manual or instrumental interference, or prolonged uterine contraction. The eighty dead children mean eighty cases either of prolonged labor, or of injudicious interference in some other way by the physician. Eighty dead children mean eighty cases in unfavorable condition for the operation. Notice that there were eighty dead children and ninety dead mothers. How nearly the numbers correspond!

The mortality in this country is attributable largely, then, to delay in operating, or to attempt at delivery with forceps, by version, or by craniotomy. Why this delay, and these injudicious resorts to other methods of delivery? First, The profession has been educated to believe that the operation is almost necessarily fatal; and statistics have been quoted to prove its terrible mortality. We have also taught, in text-books and in lecture halls, that the Cæsarean section is not an operation of election; it has been declared by authority to be the most dangerous operation in surgery. The practitioner has been demoralized into great dread of it; and he is prepared to undertake any other measure first. I have heard a once prominent teacher, now deceased, instruct his large classes, in cases of deformed pelvis, if in doubt: first to try the forceps, and if unsuccessful, to try craniotomy; and if this fails, then to try the Cæsarean operation. It would be equally, or more rational, in the management of an ovarian cyst: First, to try tapping; and if the cyst refilled, to try injecting with iodine; and if not then successful, to try drainage of the cyst; and if that failed to perform ovariotomy.

The delay is not, however, dependent solely upon the dread of the operation: It has frequently resulted from failure to recognize early the degree and character of the obstruction. In the United States large cities and large maternities are few, and medical schools are numerous, and the lectures are too limited in time, and students are taught but little of pelvic deformities and of obstruction in the soft parts. Pelvimetry is almost a closed book, and most young men, entering into private practice, have never seen even a normal labor, nor measured a pelvic canal. Is it surprising that, even later in life, they often cannot determine the degree of pelvic contraction, or of obstruction, when such exists? They cannot recognize the indication for the Cæsarean operation, hence, in part, the delay; or resort to forceps, version, or craniotomy.

The recent European success cannot be ascribed solely to the Sänger method of treating the uterine wound. Notice that in thirty-three operations by this method, in Europe, thirty-one children were saved. What does this indicate? It demonstrates that the operations have been performed early in labor; that forceps, version, and craniotomy had not been resorted to. It demonstrates that the obstetrician recognized promptly the degree of contraction, or character of obstruction; saw the indication and had sufficient confidence in the result to resort at once to Cæsarean section. The saving of 78 per cent. of the woman, under the Sänger method, does not mean that this result was attained merely because of that invaluable method; for in this country there have been five Sänger operations, and all have terminated fatally.

I submit that, in this country, we must recast our views and our teachings as to the dangers attending the Cæsarean operation, and as to the indications for its performance. Medical students must be instructed more fully in the recognition of degrees of pelvic obstruction, so that it will not be necessary for the physician to learn through delay, or through failure with forceps, version, or craniotomy, that a Cæsarean section is demanded.

In the performance of the Cæsarean operation, I will also submit a few essential rules:

1. At once carefully determine the degree of obstruction, and operate early in labor—*i. e.*, as soon as the os is sufficiently dilated to permit drainage of the lochia, and before the rupture of the membranes. Delay is fatal.

2. Operate with full antiseptic precautions; for, of all abdominal operations, no other demands so absolutely that asepsis should be secured; but the spray over the abdomen is unnecessary.

3. Control hæmorrhage by compression of the cervix, either manually or with rubber tubing, but preferably manually.

4. Introduce numerous deep and superficial sutures so as to approximate accurately the muscular walls and serous surfaces of the peritoneum; but do not carry the sutures into the endometrium. The removal of a section of the muscular wall is unnecessary.

5. Carefully protect the peritoneum from contact with fluids, and make a careful toilet of that membrane, if perchance it has been soiled.

6. Administer ergotine hypodermically at the beginning of the operation.

[Remarks of Dr. Barnes, in the discussion on the paper of Dr. Meadows, which he read before the British Gynæcological Society 13th October, 1886, in which he favored the total abolition of craniotomy:]

Dr. Barnes said he disclaimed the part of apologist of craniotomy. He earnestly hoped the time would come when this revolting operation might be abolished. He had worked hard towards this end. Some of the most trying hours he had ever spent had been caused by the conflict between the duty of saving a mother at the sacrifice of her child, and of seeking the safety of the child at the imminent risk of the mother's life.

The problem would be nearer solution could we bring the Cæsarean section in some form to such perfection that the mortality attending it would be reduced to the mortality attending craniotomy. And here he disputed the validity of the statistics cited by Dr. Meadows. Dr. Meadows put the mortality of the Cæsarean section according to Sänger's method at 20 per cent. Admitting this for the purpose of argument, and admitting further that the operation might be so improved as to attain an even smaller mortality, he would still most emphatically protest against the statement that the necessary mortality attending craniotomy approached 20 per cent., or even 5 per cent., excluding the cases of extreme pelvic contraction which forbade the hope of extracting the child after craniotomy, and which all acknowledged should be treated by the Cæsarean section. Craniotomy, done under fair conditions, such as are postulated for the Cæsarean section—that is, done at a chosen time with due skill—did not involve any maternal mortality. This being so, we were driven to fall back upon the long-recognized claim of the mother to be first considered. Now, assuming that twenty mothers out of one hundred, or even ten, or even five, were sacrificed to the Cæsarean section in order to save, say ninety children, might not those doomed mothers rightly plead their prior right to be saved? Her life is in our hands; the circumstances hardly admit of her forming a just judgment. We have to act for her, and are we not bound to do our utmost to save her?

On the other hand, the case for the child is undoubtedly strong. By Cæsarean section there is a strong probability of its survival; under craniotomy it is certainly destroyed. But this does not represent the whole case. This is what happens: A woman, with a minor degree of contracted pelvis, not admitting of the delivery at term of a live child, is delivered by craniotomy. She recovers, and time after time in subsequent pregnancies, labor being induced at seven or eight months, she bears a live child. Add these children saved to the mother, and it might happen that the aggregate lives, maternal and infant, saved by craniotomy would compare favorably with the aggregate saved by Cæsarean section.

Undoubtedly very much had been gained, and much more would be gained, in the direction of lessening resort to craniotomy. Still he feared he must feel that the abolition of craniotomy was as yet an aspiration, and not an accomplished fact.

THE TREATMENT OF CROUP WITH DIGESTIVE SPRAYS.

BY GEORGE MINGES, M.D.,

OF DUBUQUE, IOWA.

Pseudo-membranous laryngitis is always a grave affection, even under the most favorable circumstances, and any treatment which promises even slightly to diminish its mortality, is worthy the earnest attention of practical physicians. Four thousand

deaths are said to occur from it annually in England. If I offer here the tedious description of a series of cases treated by a method not new in principle, and attended by results not very brilliant, I hope that my communication may be received in a scientific spirit. I lay claim only to great perseverance in the treatment described, and I hope to prove that its details are readily carried out by parents, and kindly submitted to by the little patients.

I have treated sixteen cases with inhalations of solutions of one or the other of the digestive ferments, thrown in a spray by a steam-atomizer. The list includes sporadic, sthenic cases, as well as those of an asthenic nature, secondary to diphtheria of the fauces, and one secondary to measles. Two successful cases in which I at the time diagnosticated croup below the vocal cords, I have decided to ignore in estimating the results of treatment, having probably misinterpreted the phenomena, although Gerhardt states that all the symptoms may be absent in diphtheritic laryngitis, while they may be present in other diseases causing laryngeal stenosis. One fatal case I must also exclude, patient having been in the asphyxic stage when treatment was begun.

Let us now see how far the diagnosis can be substantiated in the remaining thirteen cases, by examining the following points:

1. The Presence of Membrane in the Fauces.—There is great diversity of opinion as to its significance. Most of the older authors, and most American and British authors to-day, recognize two distinct diseases, true membranous croup and laryngeal diphtheria; yet most of these even, and among others Wagner, admit the presence of an exudation in the fauces also in the former disease, although secondarily; while many unicists, among them Steiner, admit the existence of a localized laryngeal diphtheria. Niemeyer, a dualist, takes it for granted that the stenotic symptoms are caused by a coexisting laryngeal membrane when an exudation is visible in the pharynx. With Squire a single point of pharyngeal exudation decides for diphtheria of the larynx, while Monti, although he believes croupous laryngitis to be much rarer than diphtheritic, if I understand him correctly, thinks that croupous laryngitis may occur as a complication of faucial diphtheria.

In seven of my cases a distinct membrane was seen in the fauces. Now, we may think it tolerably certain that a pseudo-membrane caused the obstruction in these seven cases at least; but the coexistence of catarrhal laryngitis with pseudo-membranous pharyngitis, first described by Trousseau, which J. L. Smith thinks must be rare, is said by Guersant to be quite frequent among the children of the upper classes in Paris. I can see how the hyperæmic swelling surrounding a diphtheritic patch might extend to the epiglottis or vocal cords and produce symptoms of laryngeal stenosis, and I can also imaging the existence of a very small spot of exudation in the larynx without giving rise to much obstruction, so that any treatment would seem to abort the disease.

2. Dyspnœa.—Unless we have watched the development of croup secondary to some other disease, we generally find the dyspnœa alarming at our first visit, but from it alone we cannot always differentiate betwen true and false croup. Loud stridulous respiration and suffocative attacks are present in both forms. According to Steiner the dyspnœa is never so extreme in the latter, and the suffocative attacks occur earlier, although not so frequently as in the former disease. In all of my cases the dyspnœa was marked, but of course not so extreme in the favorable cases, where treatment was begun early. Jacobi lays particular stress upon its presence during expiration as well as inspiration in membranous croup. As my attention had not been directed to this point until lately, I failed to make any observations regarding it in my cases. The respirations were generally not much increased in frequency, and in proportion to the frequency of the pulse they were retarded. When they became very frequent, I took it for granted that the bronchi or lungs had become involved, and accordingly gave an unfavorable prognosis. The only measure which I know of the amount of stenosis is inspiratory recession of the epigastrium and attachment of the diaphragm. It was present to a greater or less extent in all of my cases, even in the two which I have excluded from the statistics as having probably been cases of false croup, although Meigs and Pepper say: "The persistence of a deep sulcus around the base of the chest and of recession of the lower end of the sternum and epigastrium during inspiration, for even a short time, is in the highest degree characteristic of the presence of false membrane in the larynx."

3. Aphonia.—According to Loomis and Steiner, the voice never becomes whispering or completely extinguished in spasmodic croup. According to Meigs and Pepper, it rarely does in false croup, while it always does in the membranous disease. The cough is also affected by the aphonia in true croup. Meigs and Pepper compare it to the sneezing of a young kitten, and say that they "have never heard this in catarrhal laryngitis, no matter how severe." The aphonic cough was present in all of my cases, even in the two which I have decided to exclude, but in these two the speech and cry were not at all hoarse. The so-called "croupy" or "brassy" cough which Sir Thomas Watson describes as characteristic of croup, I regard, with Trousseau, as a sign of the *absence* of membrane in the larynx. I have met it in numerous cases of spasmodic laryngitis, but only once in true croup, and there its sudden appearance at night at first led me into a mistaken diagnosis.

4. Totality of Symptoms.—Having seen that no one of the above-mentioned symptoms can be regarded as strictly pathognomonic of pseudo-membranous laryngitis, let us consider them jointly. Niemeyer says: "They can all be present temporarily in catarrhal swelling of the vocal cords." In that case, however, the symptoms are generally alarming from the start, the onset almost always being at night, and subside by morning, while pseudo-membranous laryngitis, as a rule, comes on insidiously, increasing for several days, and is not attended to until a dangerous stage has been reached. Hartshorne cautions us always to "pay prompt and careful attention to a day-time bark *with* fever." Ziemssen states that in

pseudo-croup recovery takes place by the ensuing morning. Gerhardt, in the absence of pharyngeal implication and glandular enlargement, would "rest the diagnosis for the first few times on the efficacy of remedies and the course of the disease, remembering that an exudation lasting more than thirty-six hours speaks decidedly for diphtheria, while in false croup complete well-being has occurred as early as the next morning." Meigs and Pepper recognize a severe form of spasmodic croup, with all the symptoms of the true disease, but they state that its duration is rarely longer than two or three days, that of true croup rarely less than six, often eight or ten days.

In my successful cases, after slight hoarseness of three or four days' duration, the alarming symptoms lasted at least four days, in one case over a week, with but slight remissions in the day-time, and nocturnal exacerbations of dyspnœa occurred for a week or more longer. Loomis says: "If recovery takes place, it is slow, weeks often elapsing before the voice returns, during all of which time the patient is liable to severe attacks of dyspnœa." Cormack seems to think that all the successful cases described in the classic works of Home and Cheyne, and many of the fatal ones, were cases of infantile laryngitis, the greater severity of symptoms in children as compared with those in adults being due to the narrowness of the glottis in the former. This theory is supported by the laryngoscopic investigations of Rauchfuss and Dehio, which showed that in acute laryngitis with œdematous infiltration below the vocal cords, especially where the cords are inflamed and swollen, there is laryngeal stenosis with the same dyspnœa, cough, and loss of voice that we have from membranous obstruction of the rima glottidis. Flint and Meigs and Pepper also think it necessary to see the membrane, either *in situ* or in the expectorated or vomited matters, in order to make an absolute diagnosis. I must confess that this requirement was not met in a single one of my cases, but I thought it possible that the membrane had undergone molecular disintegration under the digestive influence of the spray.

Should a supposed catarrhal croup turn out in reality to be a pseudo-membranous laryngitis, one consolation remains. We can smooth over our mistake by citing Loomis and Hartshorne as authority that the former disease can develop into the latter, suppressing the fact that most authors state that children subject to spasmodic croup rarely become affected with the membranous form.

Having shown that an absolute diagnosis could not be made in any of my cases, and leaving it to you to estimate the probabilities in each individual case, allow me to pass to the consideration of my method of using the peptonizing ferments. I claim no originality, but think I have used digestive sprays since a time when they were but little mentioned in current literature. About five years ago I read somewhere that Rossbach (?) was using a spray of papayotin solution to dissolve false membranes. I utilized the principle in the first case of croup, and that a bad one, coming under my treatment, but substituted pepsin for the expensive papayotin, and as patient recovered, I have used the method ever since, but of late in a modified form.

The mixture was prepared by adding grs. x Jensen's pepsin, dissolved in glycerine, to oj water, and acidifying with ʒss lactic acid. This solution, thrown into a spray by a Codman and Shurtleff steam atomizer, was directed towards the child's mouth and nostrils continuously, day and night, until the worst danger seemed over, generally about a week, and after that during the nocturnal exacerbations for about a week more. Of course, it is not probable that much of the fluid reached the larynx, but it is an open question whether pepsin acts by its quantity, or only in a catalytic manner. Dr. Henry Dwight Chapin has shown by experiments with trypsin solutions, that the spray dissolves membranes relatively more quickly than immersion.[1] The person using the atomizer must be constantly on the alert to follow any movement of the patient's head and keep the spray playing on the mouth and nostrils; the child then becomes restless, opens its mouth to cry, takes a deep inspiration, and the cough proves that the fluid enters the larynx. Any one can satisfy himself of this by trying it on himself. Children, after a short time, do not mind the spray much, and in all except the most fulminant cases the relief has been marked and prompt. The child usually soon falls asleep, and at this time the spray can be used continuously without trouble. It is doubtful whether any of the fluid enters the respiratory passages during sleep, but the steam alone must do considerable good. If the dyspnœa again increases, the child awakes, and, if necessary, can be made to breathe through its mouth for awhile. Older children sometimes ask for the spray, and one (case 8) even for some time after full recovery, could be put to sleep only when the "little stove" was burning beside his crib.

If the membrane form again faster than it can be digested, the relief is only temporary, and after the stenosis has become extreme, there is not enough air-current through the trachea to carry the spray where it is needed, but after the performance of tracheotomy or intubation of the larynx per os, digestive sprays ought to be of immense benefit.

One drawback of the acid pepsin spray is that it causes considerable erythema, and sometimes swelling, of the face. This can be avoided to a certain extent by anointing the exposed surfaces with cosmoline and covering the eyes with a cloth. In my last four cases I have obviated this difficulty by substituting an alkaline pancreatic solution, prepared as follows: ℞. Ext. pancreatis (Fairchild Bros. and Foster's) ʒj; glycerine, q. s. ut. ft. sol.; nat. bicarb., ʒij; sol. thymol (1:1000), ʒiv (to prevent decomposition of the ext. pancreas, which occurs very quickly); aquæ, q. s. ad. oij, filtered. These solutions I have used in large quantities, generally a quart during the day, and an equal quantity at night, and sometimes much more. Perhaps their strength could be increased with advantage.

Dr. Henry Dwight Chapin describes a hand-ball atomizer with tongue-depressor, combined in one instrument, by means of which a concentrated solu-

[1] Medical Record, vol. xxvii, p. 257, March 7, 1885.

tion of ext. pancreatis, gr. xv: ℥j, can be thrown more directly into the larynx for a minute or so every fifteen minutes.[2] In severe cases this might be occasionally used together with the constant spray from the steam atomizer. I have never had any trouble in getting parents to persevere with the treatment, but in one case, where, after the child had materially improved, even at night, the suction tube became stopped soon after my morning visit, by the scum always present in the pancreatic solution, and by evening the child was past all hope. I had explained to the parents how to cleanse the tube with a small feather from the large end, but, not being very intelligent, they failed in their attempts (case 15).

In New York they use a trypsin-spray, and according to Dr. B. M. Van Syckel, this ferment can be extemporized as follows: Add 5 grammes (ʒj grs. xvij) ext. pancreatis to 500 cc. (℥j ʒvi) of a 1 per cent. solution of salicylic acid, and digest in a water bath at 37° C. for four hours; filter, and make slightly alkaline with bicarbonate of soda.[3]

Of course, other treatment must not be neglected. I have hitherto used a slice of salt pork externally, but think I shall hereafter try hot poultices. The body must be protected from the wet spray by being wrapped in shawls or oil-silk. Increased secretion diminishes congestion and aids in loosening the exudation. This I have striven to promote by pilocarpin mur. in doses of gr. $\frac{1}{48}$ to gr. $\frac{1}{24}$ q. h. or q. 2 h., day and night. To the pilocarpin mixture I add a few drops of dilute muriatic acid to aid solution, tr. ferri chlor., am. mur., and brandy to forestall the depressing effects of the pilocarpin. As I have once or twice seen considerable temporary collapse from the pilocarpin, I think I will omit it in asthenic cases occurring during epidemics of diphtheria. But it is necessary also to *prevent* the new exudation of pseudo-membrane. This I have aimed to do with small doses of calomel, gr. $\frac{1}{48}$ to gr. $\frac{1}{4}$ q. h. or q. 2 h. day and night, given between the doses of pilocarpin. If there is much fever, I give occasional doses of quinine. Nourishment is generally taken in sporadic cases, but must be insisted on in the asthenic, combined with stimulants. I do not believe in frequent large doses of mercury, although I generally begin treatment with a gr. v–gr. x dose of calomel, which I repeat once or twice during the following days. Mercury never salivates small children, but often produces sudden and profound asthenia without salivation, as the personal experience of several physicians testifies.[4] The treatment of this dread disease must be active from the very beginning, but should never be reckless. I would never dare to use the heroic doses of corrosive sublimate advocated by Jacobi, gr. ss. daily for many days in an infant of 1 year. J. Lewis Smith, a dualist, uses large doses of mercury in croup, but avoids them in asthenic diphtheritic cases. Opiates I would only give when the cough is distressing, and then only in small doses, as they have seemed to me to hasten the occurrence of the asphyxic stage. I prefer Dover's powder, as it keeps up a gentle diaphoresis, and stimulates the secretion of mucus.

Emetics I have hitherto studiously avoided, as I once saw a patient, whom I treated before I had begun to use pepsin inhalations, die of suffocation during the retching produced by an emetic. However, I shall hereafter try an occasional dose to expel mucus and membrane disintegrated by the spray and collected in the air passages, because Meigs and Pepper claim that of thirteen cases where they pushed emetics, ten recovered without, and one with, tracheotomy, while of eight others all but one died.

Where it is often impossible to make a definite diagnosis, we must of course be careful in estimating the results of treatment. On the point of prognosis there is even greater diversity of opinion than on that of diagnosis. Wood, of Philadelphia, gives the mortality of croup as low as 2 per cent. This is obviously a different disease from that described by Ware, who states that of twenty cases nineteen die. Then we must bear in mind that many authors recognize two distinct diseases, the one membranous croup pure and simple, the other laryngeal diphtheria, of which they find the latter much more fatal than the former. Monti has seen spontaneous recovery in not more than 2 per cent. of his cases of idiopathic croup, 2 per cent. more yielding to the lactic acid spray, while 50 per cent. recovered after tracheotomy; but in descending croup with enlarged glands and constitutional symptoms, he does not think that more than 25 per cent. can be saved by the operation. Even those writers who maintain the identity of croup and diphtheria, admit that sporadic cases recover more frequently than those occurring during an epidemic; but in their treatment they generally include tracheotomy. Thus, of a large number of cases treated by Steiner only three recovered without tracheotomy; with the operation his mortality was 60 or 70 per cent.

I have been able to obtain consent to perform tracheotomy but once, and then only after the child was moribund. Intubation by Dr. O'Dwyer's instruments would probably be more readily allowed,[5] and I can conceive it possible that in one or the other of my fulminant cases the operation might have gained time for the spray to act. Dr. F. E. Waxham, of Chicago, writes me that of fifteen cases treated by him by intubation, seven have recovered.

Meigs and Pepper have had sixteen deaths out of thirty-five cases, their favorable results apparently being due to the free use of emetics.

Secondary croup occurring in the first week of diphtheria seems to be much more fatal than that occurring in the second or third week. Dr. J. Lewis Smith thinks it is scarcely possible to cure more than one-eighth of the cases of the former, while of the latter one-third may be saved by the early and constant or almost constant use of trypsin inhalations.[6]

Now as to my own results. Before I began to use the treatment above described, I had three cases of croup, all fatal; but they were all very severe and

[2] Medical Record, vol. xxvii, p. 257.
[3] Medical Record, vol xxvii, p. 207, Feb. 21, 1885.
[4] "Diphtheria and its Management," by Dr. Ed. Winters in Med. Record, vol. xxviii, p. 622.

[5] Med. Record, Feb. 21, 1885, vol. xxvii. p. 207; also Jour. Amer. Med. Ass'n., May 2, 1885, vol. iv, p. 498, and ibid., Oct. 24, 1885, vol. v, p. 460.
[6] Med. Record, vol. xxvii, p. 543.

apparently infectious, two occurring in families in each of which another child died of the same disease, either just before or soon after, and the third in a girl of 15, convalescing from typhoid fever, infected by a brother with diphtheria.

Of the thirteen cases which I will utilize in estimating results, six recovered. All of these ran a slow and protracted course, and hence gave the treatment some chance to succeed. Of the seven fatal cases, all but one (case 7) ran through the different stages very rapidly. Of seven cases where a distinct membrane was seen in the fauces, three recovered. Loomis says: "When the diagnosis is based upon the presence of the membranous exudation in the tonsils and epiglottis, recovery seldom occurs."

I have tried to compare my results with those of others reported in the Dubuque Health Office. Here again we must bear in mind the probability of frequent errors of diagnosis, especially by irregular practitioners. and of fatal secondary cases simply reported as having died of the primary disease. On the other hand, cases ending in recovery may have been not reported at all. I will select the report of the health physician for the year ending February 28, 1883, as the disease was then rather prevalent, and hence more liable to be fatal, and errors of diagnosis less likely to occur. During that year there were reported 37 cases of croup, with 21 deaths. Of these, 32 were treated by other physicians and by methods unknown to me, with 19 deaths, a mortality of 60 per cent; 5 were treated by me as above described, with 2 deaths, a mortality of 40 per cent. Of the cases which recovered, one occurred during convalescence from measles (case 5), when, according to Steiner, true croup is much more common than false, while the reverse holds true in the beginning of measles.

Both Steiner and Monti lay stress upon the rarity of recovery from croup occurring in the desquamative stage of measles, the latter never having seen one get well. In the second case (case 6) the extensive pharyngeal exudation, which had been seen by the mother, had disappeared when I was first called, but the submaxillary glands were much enlarged. The third case (case 8), may have been one of severe spasmodic croup, although the long duration of the prodromal stage (four days), the first occurrence of suffocative attacks at noon, the severity of the symptoms, and the slow recovery, speak rather for pseudo-membranous obstruction. Of the fatal cases, the first (case 4), was fulminant, and time was lost by treating it at first for spasmodic croup; in the second (case 7), the treatment had a pretty fair trial, but failed to save the patient.

Following is a brief review of my cases:

Case 1.—Frank P. Treated him for sore throat, October 16, 1881, with cinchonidiæ sulph. and tr. fer. chlor. internally, and had throat swabbed with acid, carbol. and liq. ferri subsulph. in glycerine. Exudation in pharynx on 20th. Now added to other treatment pilocarpin mur. gr. $\frac{1}{10}$ q. 2 h.

October 22, add gr. iv quiniæ sulph. t. i. d. for fever. Considerable cough, for which gave morphine and āā hydrocy. dil.

October 24. Brush throat with pure tr. ferri mur. During following night laryngeal stenosis suddenly developed, while membrane in fauces had disappeared. Continuous pepsin-spray day and night.

October 25. Dr. J. M. Boothly saw patient in consultation in evening, and at his suggestion, am. mur. and cinchonid. were given q. 2 h., alternating with these calomel gr. $\frac{1}{6}$, tart. em. gr. $\frac{1}{15}$ pulv. Doveri gr. ss q. 2 h. Pilocarpin and spray continued. Stimulants and milk frequently repeated.

Condition remained critical for several days. Some improvement on 28th, and considerable by 30th, but now considerable cough. Spray continued some time longer, at last only at night. Aphonia remained for some time.

Case 2.—Mary U., æt. 4 years, taken with diphtheria on Dec. 2, 1881, while a brother was convalescing from sore throat with enlarged submaxillary glands. Cinchonid. sulph. gr. iv t. i. d., pilocarpin gr. $\frac{1}{12}$ q. h. Some improvement after a few days, when, after getting up, croupy symptoms began to appear and were well developed by the 8th. Alternated with pilocarpin increased to gr. $\frac{1}{10}$ q. h., calomel gr. $\frac{1}{4}$ and pulv. Dov. gr. ss q. 2 h. Pepsin-spray continuously.

December 16. Pilocarpin q. 2 h., and tr. ferri mur. gtts. ij added to each dose.

December 19. Patient slowly improving, but still considerable obstruction at night. Added tart. emetic gr. $\frac{1}{24}$ to each dose of calomel.

December 23. Convalescent, but weakness of heart for which gave quinia, iron, nux vomica and digitalis. Spray at night for some days longer. Last child in family came down with diphtheria.

Patient got out of bed on 30th or 31st. Aphonia remained complete for about two months.

Case 3.—Olivia H., æt. 1 year 9 months. Slight exudation had existed in fauces for four days when I first saw the child on January 6, 1882. Hoarseness had been increasing about thirty-six hours, and stenosis was so marked that an unfavorable prognosis was given. Pilocarpin mur. gr. $\frac{1}{12}$, am. mur. gr. $\frac{1}{4}$ q. h. Cinchonid. sulph. gr. j q. 4 h. Continuous inhalations of pepsin-spray.

January 7. Respiration frequent, fever higher, probably indicating lung implication. Mustard poultices to chest, covered by oil-silk. Tracheotomy refused and not urged on account of probability of lung trouble. Symptoms increased in severity until death occurred December 8, 6:30 P.M. Duration of whole disease six days, of laryngitis three days, of treatment forty-two hours.

Case 4.—Peter M., æt. 3 years. Called on night of April 9, 1882. Sickness began three days ago with cough. Croup symptoms came on suddenly this evening. Thinking it a case of spasmodic croup, I vomited the child with sulphate of copper, and prescribed tartar emetic and sulphate of morphia āā gr. $\frac{1}{24}$ q. h. Much worse next morning. Cinchonid. sulph. gr. iv q. 4 h., pilocarp. gr. $\frac{1}{12}$ q. h., and continuous pepsin-spray. Patient died at 3 P.M. Duration of disease five days. Pepsin inhalations only four or five hours.

Case 5.—Francis B., æt. 3½ years. About a week after an attack of measles she was taken with hoarse-

ness, which gradually increased for several days, until when I first saw her, on July 6, 1882, symptoms of stenosis and aphonia were well marked. Cinchonid. sulph. gr. ij q. 4 h., pilocarpin gr. $\frac{1}{24}$ with tr. ferri mur. q. h. Pepsin inhalations day and night.

July 8. Added to other treatment calomel gr. $\frac{1}{16}$ and pulv. Dov. gr. ss q. 2 h., after bowels had been moved with grs. v calomel.

July 9. Ammoniæ carb. gr. j every two hours.

July 10. Convalescent. Tr. ferri mur. gtts. ij q. 2 h. Last visit on July 11, but spray used at night for slight exacerbations for a week longer.

Case 6.—Albert M., æt. 3 years 10 months. Sore throat for several days with glandular enlargement. Mother said fauces had been covered with whitish exudation, but I could see none when called on October 24, 1882. Gradually increasing hoarseness for several days, now aphonia and symptoms of laryngeal stenosis. Pilocarpin gr $\frac{1}{24}$, am. mur. gr. $\frac{1}{8}$, tr. ferri mur. gtts. ijss, q. h. Continuous pepsin inhalations. Whenever atomizer became deranged for a short time, symptoms increased in severity. Dr. J. H. Wilson saw case after two or three days and agreed in diagnosis.

October 29. Ung. plumbi iod. to enlarged submaxillary glands.

October 31. Last visit. Continue inhalations at intervals, as necessary, for a few days longer.

Case 7.—Lisetta R., æt. 4 years 4 months. Gradually increasing hoarseness and stridor for several days, until stenosis was far advanced when I first saw her on evening of December 17, 1882. Complete aphonia. Continuous pepsin inhalations. Pilocarpin gr. $\frac{1}{32}$, tr. ferri mur. gtts. jss q. h. Cinchonid. sulph. gr. j q. 4 h. Tracheotomy refused. Patient became progressively worse, respirations hurried, high fever, and death occurred at noon of December 19, apparently from lung implication. Duration of sickness one week, of treatment forty-two hours.

Case 8.—Karl H., æt. 3 years. Treated patient January 20, 1883, for cough with slight hoarseness. Did not see him again until 31st. Hoarseness rather worse, but did not think of croup. Sent for at noon of February 1, and found symptoms of croup well developed with deep sinking in of epigastrium and base of thorax inspiration. Immediately began with continuous pepsin inhalations and relief marked. Internally gr. ij cinchonid, sulph. q. 4 h., and pilocarpin mur. gr. $\frac{1}{32}$ with tr. ferri mur., am. mur., and brandy q. h. day and night. Some improvement by February 5. Added to other treatment gr. $\frac{1}{24}$ calomel and gr. $\frac{1}{8}$ pulv. Dov. to prevent reformation of membrane. Whenever spray was discontinued for a short time, symptoms increased in severity.

February 6. Omitted pilocarpin. Ferri et quin. cit. t. i. d. Atomizer was used about two weeks, but during last few days only at night when dyspnœa still existed.

Case 9.—Mathias Sch., æt. 5 years, living seven miles in country. Called on evening of February 20, 1883, and found child in last stage of croup. Did not examine fauces. Child died two hours after treatment was commenced, and after an illness of five days.

Case 10.—Leonard L., æt. 5 years 11 months. Passed through severe attack of scarlatina anginosa with faucial exudation, in 1881, making a slow recovery. Had been sick about four days when I was called on October 15, 1883, but had gone to school until the day before. Found extensive diphtheritic exudation in fauces, and laryngeal stenosis far advanced. Pilocarpin gr. $\frac{1}{24}$, tr. ferri chlor. gtts. ijss, am. mur. gr. ss q. h. Swab throat every hour with carbolic acid and liq. ferri subsulph. Continuous pepsin inhalations begun after supper.

October 16, in morning much worse. Added calomel gr. $\frac{1}{30}$, and pulv. Dov. gr. $\frac{1}{2}$ q. h. Pilocarpin increased to gtt. $\frac{1}{30}$, q. h. Distressing cough almost constantly. Still worse in afternoon. Some relief followed an emetic dose of cup. sulph. Eased cough with small doses of opiates. At 8 P.M. membrane seemed looser, wherefore gave large doses of cup. sulph., but nervous sensibility so obtunded that they would not even produce nausea, and child died in a short time, twenty-four hours after beginning inhalations, and five days from beginning of sickness.

Case 11.—Josie B., æt. 14 months. Stenosis had been gradually increasing for nine days, until I was finally called November 6, 1883, at 4 P.M. Unfavorable prognosis. Continuous inhalations of pepsin-spray. Internally, pilocarpin gr. $\frac{1}{32}$ and tr. ferri mur. gtts. ij q. h. Also, calomel gr. $\frac{1}{12}$ q. h. Eight P.M. much worse.

November 7. Grew rapidly worse to noon. Tracheotomy mentioned but not allowed until 4 P.M., when child was in asphyxic stage. Dr. J. S. Lewis was immediately summoned to assist in operation; but we feared child would die before we could get ready. A few whiffs of chloroform were given. Trachea was reached in a few seconds without appreciable hæmorrhage, and promptly opened, but in enlarging the opening the churning motion of the larynx caused the innominate veni (?) to be punctured by the knife; at any rate, considerable hæmorrhage ensued. The smallest tracheotomy tube was too small for the small wind-pipe. Immediately introduced silver male catheter, but patient died the same moment, twenty-four hours after beginning of treatment, and ten days after initial hoarseness.

Case 12.—Annie T., æt. 7 months. Scrofulous. Symptoms of croup came on January 1, 1884, at 1 A.M., after slight prodroma of several days' duration, and were well developed when I saw patient. Began immediately with continuous pepsin inhalations. Internally gr. $\frac{1}{32}$ pilocarpin q. h.

January 2 gave also calomel gr. $\frac{1}{30}$ q. h. with pulv. Dov. $\frac{1}{16}$.

January 4, added to pilocarpin mixture gr. $\frac{1}{150}$ corrosive sublimate, tr. ferri mur. gtts. ij., am. mur. gr. $\frac{1}{8}$ pro dosi.

January 5, returned to calomel. Symptoms became considerably milder from beginning of inhalations, but increased in severity whenever apparatus got out of order, which happened several times, and whenever spray was resumed stenosis became much less alarming. Gradually improved to January 8, when tr. opii deod. gtt. $\frac{1}{4}$ q. 2 h. was given for severe cough.

January 9, last regular visit for the time being, but patient was kept under observation for some time longer, during which time there was more or less dyspnœa at night, wherefore spray had to be used at those times, and always with marked benefit. Child remained sick for a long time, passing successively through whooping-cough, pneumonia, and gastro-intestinal disorders, and in August of same year had an attack of undoubted diphtheria, with glandular enlargement.

The last fact would rather militate against the identity of croup and diphtheria, provided the child had true croup in January, as the long duration of the stenosis would seem to indicate, while the sudden onset at night rather speaks for spasmodic laryngitis. Almost two years later the same child was taken in the middle of the night with croupy symptoms more alarming to the parents than those of the previous attack, but on that occasion I promptly excluded true croup, and the recovery by morning under simple treatment proved the correctness of the diagnosis.

Case 13.—Annie W., æt. 3 years. Treated her three months ago for acute Bright's disease, of which the cause could not be discovered, and two weeks ago for bronchitis, which developed into lobular pneumonia. Had not seen her for several days, as she seemed convalescent. On the night of April 8, 1885, she was taken with croupy symptoms, which the mother relieved temporarily with a dose of hive syrup. Next morning, breathing accelerated, but not stridulous. Grew worse until I saw her at 7 P.M. Considerable fever, pulse 150, respiration 57, and of the character indicating stenosis of larynx and trachea, with recession of supra-clavicular and supra-sternal fossæ, as well as of border of ribs, during inspiration. Auscultation showed only prolonged expiratory sound, no râles of any kind. Pain in throat; no exudation visible. No hoarseness of voice in speaking, but cough aphonic. Diagnosticated ascending croup, beginning in the bronchi, which has not yet reached the vocal cords. Gave grs. v calomel. Pancreatic spray to be used all night. Pilocarpin mur. gr. $\frac{1}{24}$ q. h. Breathing soon became easier, although still stenotic, and patient slept all night with the spray playing in her face.

April 9, stool at 5 A.M. At 8 A.M. respiration 54, pulse 150; no stridor, but some drawing in of diaphragmatic attachments during inspiration. Prolonged expiratory sound, some dry râles. Ate a soft egg for breakfast. Calomel grs. x, continue other treatment. Ate some dinner. Played all afternoon and had two liquid stools. Six P.M., respiration 45, dry râles, no longer recession of lower ribs. Used only steam with atomizer during following night.

April 10, patient playing, pulse 120, breathing accelerated but noiseless. Made a prompt convalescence.

The above case I will exclude on account of quick recovery.

Case 14.—Karl H., æt. 8 months, brought to office April 21, 1885, with very loud wheezing in- and expirations, little if any hoarseness when crying, but high-pitched, almost completely suppressed cough, no fever; all these symptoms of two weeks' duration. Playful during day and sleeps all night in spite of the dyspnœa. Auscultation of lungs negative, as stridor in trachea masks everything. Thinking it to be a catarrhal affection, I prescribed am. carb. and tr. sanguin. āā gr. $\frac{1}{3}$ q. 2 h., with hot poultices of linseed and mustard to chest. Wheezing soon improved some, but on 23d I was sent for in haste at 5 P.M. Wheezing much worse, with inspiratory recession of supra-clavicular and supra-sternal fossæ, xyphoid cartilage and free border of ribs. No fever. Respiration 48 to 56. In spite of this, child was excessively and wildly playful, laughing, crowing, kicking all the time, so that friends who came in thought it under the influence of liquor, although none had been thus far given. I now prescribed pilocarpin gr. $\frac{1}{48}$ q. h., with brandy, no iron, and an initial dose of grs. v calomel. The atomizer was used constantly, but at present only water was used in the spray. Calomel operated several times, and child slept well during intervals of taking medicine. Vomited once.

October 24. Respiration seemed somewhat slower, but could not be counted, as child's excitement continued. Coughing spells more severe, child turning red in the face during the attacks. Found child asleep in the evening. Respiration 32, pulse 132. Increased pilocarpin to gr. $\frac{1}{32}$ q. h. Spray used only during sleep, as child was too wild when awake.

October 25. Stridor increasing, and more like that of croup; cough more aphonic. Hands cold. As symptoms seemed to be increasing under water spray, and diagnosis of croup more certain, the pancreatic spray was substituted. Pilocarpin gr. $\frac{1}{32}$ only every two hours, as salivation has been considerable. Atomizer became deranged for awhile and cough became still more suppressed. Child was restless and feverish. Ten P.M., collapse, cold, clammy perspiration, pale, no cyanosis, ceased breathing for a few moments, then vomited large quantities of glairy mucus; after this some improvement of stridor and cough; pulse 132, full, respiration 40, more moist. Thinking that the pilocarpin had caused the depression, it was diminished to gr. $\frac{1}{48}$ q. 2 h., and the salivation then ceased, and calomel was now added, gr. $\frac{1}{24}$ q. 2 h. From this time gradual improvement to 27th, when respiration during sleep, 28; no more recession of ribs. Pilocarpin stopped on 29th, but resumed again on November 2d, as respiration again became more stridulous.

November 3. Oil-silk jacket instead of poultices.

November 7. Slight recession of ribs. Chlorate potash gr. j, q. 2 h.

November 9. Stopped atomizer. Some wheezing when awake, but breathes noiselessly during sleep.

November 13. Breathes much louder, so that the stridor can be heard through two rooms, yet no dyspnœa whatever when asleep.

November 14. Wheezing only during expiration. Sounds very much like asthma, yet the stridor is distinctly in the larynx and trachea; no râles in chest.

November 15. Has been taking iod. pot. several days, which has produced iodism.

November 18. Expiration whistling. Boils have been forming on head, and muco-purulent and bloody casts are discharged from nose.

November 20. Breathing natural.

November 28. Aphthæ of tongue.

December 1. Otitis media, which has existed for several days, has ended in perforation of left drum-membrane.

December 10. Last traces of aphonia of cough now gone.

December 12. Double pneumonia, which underwent resolution in about a week, when a boil formed in left ear and broke.

January 8, 1886. Still considerable cough and discharge from left ear.

The above case I am now inclined to consider one of chronic catarrh, beginning in the larynx and trachea, ascending into the nares and middle ear, and descending into the bronchioles and air vesicles, and the improvement following the use of the digesting spray was probably only a coincidence.

Case 15.—Julia K., æt. 1 year, scrofulous child, has been sick a good deal, almost dying of enterocolitis last August, living in the basement of a damp house. On morning of November 18, 1885, during cold, wet, foggy weather, was suddenly taken with hoarseness, which became worse by evening, with some stridulous respiration. Still worse on following morning, but quite well again in the evening. Worse again on morning of the 10th, and in the evening of same day I first saw the child. Cry and cough devoid of all sound. Had a suffocative attack in my presence, and I thought I could hear a loose membrane flapping in the larynx. Dyspnœa not very severe in intervals between these attacks. Retraction of lower ribs and xyphoid cartilage very slight. White membrane on posterior wall of pharynx. Little, if any, glandular enlargement. Cannot nurse well. Prescribed pilocarpin mur. gr. $\frac{1}{16}$ q. h., with tr. ferri mur., and an initial dose of gr. v calomel. Spray of ext. pancreatis was kept up continually, and child slept well, breathing almost noiselessly until, at 5 A.M., the suction-tube of atomizer became stopped, and by 10, at my visit, the child was again in about the same condition in which I had left it the night before. In the meantime it had nursed without apparent difficulty. I cleansed the tube, but after I left it soon became clogged again, and at 5 P.M. dyspnœa was extreme, with deep recession of xyphoid cartilage, cyanosis, apathy, child lying with its head strongly extended, as in opisthotomos. Respiration 48, pulse very frequent and feeble. The medicine had been given in only one-third of the intended doses. The child's mouth was now held open with a spoon, and the spray used continually by myself for an hour, at the end of which time respiration seemed much moister. Tracheotomy not allowed. The parents kept up the spray until 8:30 P.M., when the child died quite easily, apparently of asthenia, twenty four hours after the beginning of treatment.

Case 16.—Fannie T., æt. 6 years, began to have sore throat on morning of December 5, 1885, a severe snow storm having occurred on previous evening. Better on 6th. Went to school on 7th. Very slight hoarseness on evening of the 8th. When I first saw patient on 9th, at 10 A.M., there was complete aphonia, breathing was beginning to be stridulous, and there was some inspiratory recession of free border of ribs. Pulse 120. Small spot of diphtheritic deposit on left tonsil. Ordered salt pork to be tied around throat, and gr. $\frac{1}{16}$ pilocarpin mur. to be given every hour, after an initial dose of grs. x calomel. The pancreatic spray was used so diligently that by 7 P.M. 2¼ quarts of the solution had been atomized. Respirations were then 28, pulse 132. Considerable perspiration, ptyalism, and some vomiting. Had eaten some potatoes and squash for supper. Almost incessant cough, for which gave gr. $\frac{1}{16}$ pulv. opii. By 8:30 P.M. pultaceous deposit had spread over whole of left tonsil. Pulse 160, respiration 40. Discontinued pilocarpin. Calomel has operated several times. Thought I heard a creaking inspiratory râle, which to me indicated extension of the disease into the bronchi. Little cough; sleeps by snatches.

Dr. J. H. Lewis was called in consultation. Calomel gr. ¼ q. h. Brandy ℥ j, and quinia sulph. gr. ij q. 2 h. Inunctions of oleate of mercury over larynx q. 2 h. Room to be filled with steam by immersing red-hot flat-irons in vessels of water. Spray to be used for a minute every fifteen to thirty minutes, patient breathing through mouth. Tracheotomy not allowed. Was called at 1:10 A.M. and found patient asphyxiated, unconscious. Died quietly in a few minutes, fifteen hours after beginning of treatment, and five days after first symptom of sore throat.

Having given a sketch of the cases treated, and recognizing the imperfections of which I was guilty in their observation, I would like to urge physicians to give the digestive ferments a fair trial in conjunction with such other treatment as the individual cases may demand. If it would seem impossible for the extract of pancreas or trypsin to have any solvent action in the small quantities inhaled in this way, how much more inert must be the lime-water spray used in the same manner, and if the former be not more active than the latter, it has at least the virtue of being equally harmless. Should any one, stimulated by this report, devise a readier means of bringing the digestive fluid in contact with the false membrane, making the method applicable also to fulminant cases, and doing away with the reckless and dangerous doses of mercury now in vogue, the object of this paper will be accomplished.

Dubuque, Iowa, January, 1886.

LUPUS VULGARIS.

Read before the Chicago Society of Ophthalmology and Otology, on December 14, 1886,

BY LYMAN WARE, M.D.,

MEMBER ILLINOIS STATE MEDICAL SOCIETY AND CHICAGO MEDICAL SOCIETY; SURGEON TO THE ILLINOIS EYE AND EAR INFIRMARY; OCULIST AND AURIST TO THE ORPHAN ASYLUM.

History.—Lupus vulgaris is no new disease, but on account of its frequent occurrence and, until the past few years, its almost incurability, I beg to call the attention of the faculty to a few cases which have come under my observation. Before referring to the history of these cases or submitting to your examina-

tion two or three patients who have kindly consented to appear before you, I would, in a few words, recall to your minds some notable features of this disease:

There is no doubt that the disease was generally recognized at least 500 years since, and designated "lupus." That in many cases it may have been confounded with carcinoma, by ancient writers, is highly probable. Willan was the first to apply the term lupus exclusively to certain forms of ulceration about the face. Bateman made no actual distinction between the effects and course of carcinoma and lupus, except that he considered the latter cured by arsenic, the former not.

Symptomatology.—Lupus varies much in appearance, according to the age of the patient, constitution, state of health, kind of diet, and the part affected. At first it appears in the form of tubercles, either isolated or in groups, which may be deeply imbedded in the true skin, beneath it, or quite superficial, and always spreads by infecting the adjacent skin. It is never congenital, though it may appear early in life. Unchecked by treatment it inincreases more or less steadily for many years. At times the disease may appear checked for years, when it will again break out with renewed vigor. In old age the severity of the disease is often diminished. Lupoid tubercle may terminate either in involution, leaving the skin atrophied and glistening like a scar, or in disintegration and ulceration.

It may occur anywhere upon the body, although most frequently, by far, upon the face. The cases which have come under my observation have originated on the lower lid, extending gradually to the eye and nose. Lupus of the alæ nasi and the tip of the nose assumes the dissiminatic form. The tubercles are prominent, and coalescing, form large, irregular protuberances. Lupus on the lower lid produces, first, complete ectropion, in consequence of cicatricial contraction of the skin; then gradually extends to the palpebral conjunctiva, the ocular and deeper tissues of the eye, involving at last the entire orbit. As the disease involves the conjunctiva it imparts to it a dark, reddish-brown color, and is decidedly trachomatous in appearance; later it becomes smooth, glistening and atrophied.

Etiology.—Many theories have been advanced respecting the cause of lupus. Both ancient and modern writers refer it to scrofula or syphilis. Although the terms scrofula and scrofulous have been used for ages by physicians as well as by laymen throughout the entire world, it must be confessed we have no very clear idea as to what scrofula is, or to what exact condition scrofulosis should be applied. In my younger days I was taught that scrofula, being a literal translation of the Latin *scrofa*, swine, was a disease common to those who eat pork. I have since thought that such an explanation was unfounded, and that the term "scrofula" originated in the fact that the submaxillary furrow, which does not exist in well-fed swine, was obliterated by glandular enlargements. Some who hesitate to use the word scrofula, designate exactly the same condition by the term "struma," or "strumous diathesis."

Although we may not be able clearly to define the term "scrofula," or "struma," yet in a case of indolent swelling of the lymphatic glands, large lips, flabby, soft muscles, with a feeble constitution, and where the slightest irritation produced chronic inflammation, followed by suppuration, we would not hesitate to designate such a condition as scrofulous, or strumous. In most of the cases that have come under my observation such a condition was not present, nor was there any history of syphilis. Some writers, while not considering lupus a syphilitic disease, think that it shows a syphilitic taint, yet I was not able to discover a single symptom that pointed toward hereditary syphilis. The patients generally were in excellent health, appetite and digestion good. The more I compare the two conditions the less resemblance I find. Lupus is an exceedingly chronic, indolent disease, arising from small tubercles imbedded in the substance of the skin, does not become rapidly worse, may persist for years without producing constitutional disturbance, and is wholly unaffected by anti-syphilitic treatment. It is also essentially a disease of early life, seldom making its appearance before the third year and hardly ever after the twentieth or twenty-fifth. When it appears later, it will generally be found to be a recurrence and not a primary attack.

The treatment of lupus rather indicates that it may be allied to the malignant diseases, a connecting link, as it were, between the non-malignant and the malignant.

Volkmann has recently laid great stress upon the fact that every portion of the diseased tissue should be most thoroughly eradicated by the scoop, and the same thing has been taught and attempted for many years by means of the knife, the caustic, or the galvano-cautery. It has long been insisted upon that whatever method be adopted it must be thoroughly and unflinchingly carried out, and repeated, if necessary, from time to time, according to the exigencies of the case, or its tendency to relapse. Most authors have considered it necessary to treat the disease locally, some by constitutional remedies only.

In the cases reported I have tried each method separately, then unitedly, and have no hesitancy in saying that when the local treatment was combined with proper internal medication, the advance toward recovery was much more rapid and satisfactory than when only one method was followed. The internal treatment consisted principally of tonics, as quinia, iron, strychnia, phosphates, and particularly arsenic, either in the form of arsenious acid or Fowler's solution, and special attention to a plain, nutritious diet, and out-door life. The local treatment consisted in the complete destruction of the existing lupoid tubercles, in whatever state of development, by means, first, of the scoop, or Volkmann's curette; then by a thorough and prolonged application of pyrogallic acid in crystals.

In some cases, after removing the superficial layer, consisting of morbid products, scales, secretions, etc., a pledget of absorbent cotton was saturated in a four per cent. solution of cocaine and firmly held on the diseased tissue. In other cases, where the lupoid tubercles were deeply imbedded or where the disease

was at all extensive, general anæsthesia was first induced. The patients never complained so much of the acid application as of the curetting or scooping out of the lupoid mass. The pyrogallic acid uniting with the blood and lupoid tubercles, produces a thick, brownish, syrupy substance, and as long as this is produced the acid should be applied.

Jarisch, of Vienna, was the first to call the attention of the medical profession to the local use of pyrogallic acid, though it had long been used in the arts, particularly in photography. Dr. A. Vesey (*Dublin Journal Med. Science*, 1878), observing its remarkable astringent effects on the hands of those engaged in photography, was led to employ it in internal hæmorrhage, and especially in the hæmophysis of phthisis, in which he found it very efficient in grain doses frequently repeated (every hour or oftener.)

Jarisch first used it in the form of a ten per cent. ointment, with vaseline. Besnier, of Paris, makes use of a saturated solution of the acid in ether, applies it with a camel-hair pencil to the diseased portion, and at once covers this with a layer of traumaticine. These applications are repeated until the disease is thoroughly eradicated.

Schrummer, of Buda Pesth, after destroying the lupoid tubercles by means of the ten per cent. ointment already mentioned, prevents a return of tubercles in the cicatrix, which is of frequent occurrence, by the application of mercurial plaster.

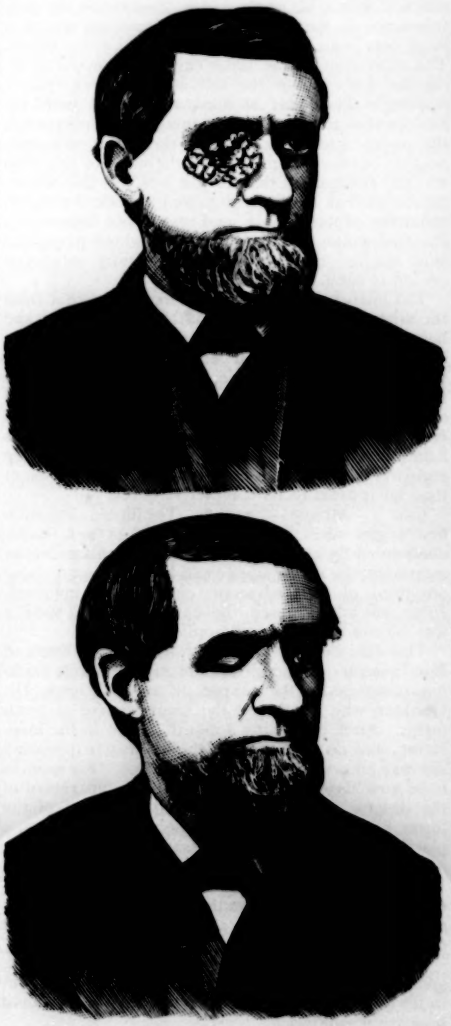
In some few cases in which pyrogallic acid has been used extensively and long-continued, poisoning has resulted; but when its use is restricted to the removal either of lupus or epithelioma, it is not at all likely to be complicated by any such unpleasant result. In cases of poisoning the kidneys are first affected, prostration and febrile disturbances appear, the urine becomes brownish or olive-green in consequence of hæmoglobinuria, the skin tinged with green, and a glairy mucus is vomited.

Dr. Charles W. Allen, of New York, has reported a number of cases of lupus treated by pyrogallic acid, and has expressed himself as most pleased with its effect. He made use of both the powder and ointment of varying strength, made with vaseline. He considers the acid of great value because it specially attacks the lupoid tubercles, induces destructive ulceration in and about them, leaving the healthy tissue unchanged.

In any preparation of pyrogallic acid care must be exercised not to combine it with an alkali, which would neutralize it; nor with a metal, which it would reduce.

Six cases have been treated, but in Case 1 iodoform in powder was used instead of pyrogallic acid.

Case 1.—Jas. M., æt 45 years. Was first attacked twenty years since while in the U. S. Army. The disease made its appearance at the inner canthus of R. eye. He attributed it to an injury received while riding under a tree and being hit by an overhanging limb. The disease gradually extended to the under lid, involved a portion of the upper, encroached upon the cheek, and finally the deeper tissues of the eye itself. The patient was given ether and the diseased mass thoroughly removed by means of the curette, and the wound dressed with powdered iodoform. Two weeks subsequently, a few lupoid tubercles again made their appearance at the inner canthus, but were easily removed by the curette without anæsthesia. The patient made a rapid recovery, and remained under observation about six months, and no relapse occurred. He assurred me on leaving, that should there be a return of the disease he would surely let me know. He was placed from the first upon Fowler's solution of arsenic and advised to continue its use for at least six months.



The cuts shown give a good idea of the appearance of the case before and after treatment.

Case 2.—F. W., æt. 50., stone cutter; residence 441 Desplaines St., Chicago. Twenty years since a small tubercle about the size of a pea made its appearance upon the left ala nasi. It slowly increased until it attained the size of a large bean, ulcerating and extending at its edges. A few years later lupoid tubercles appeared at the inner canthus of the L. eye, and gradually extended to the lower lid.

Mr. W. had submitted to a great variety of treatment, which had only partially kept the disease in check. When he came under observation the protuberance on the ala nasi was, at least, the size of a large lima bean, dark-red and ulcerated at the edges. The ulceration at the inner canthus occupied an irregular space, about the size of a ten cent piece. Although there was at least an inch of perfectly healthy skin and tissue intervening between the two, the patient was confident that there was some connection between them. When the ulcer of the nose was red and painful that of the eye was also worse. As the patient had considerable fortitude the lupoid tubercles of both nose and eye were thoroughly curetted without the aid of ether, and the pyrogallic acid applied until the brownish syrupy substance ceased to form.

The history of this case differed somewhat from the others, in this respect, that for years there had been more or less pain. In fact, of late, the pain had been so great that it was impossible for him to work at his trade or sleep, sometimes for several nights in succession. The operation relieved him completely of the pain and he was able to sleep, and returned at once to his work. Several operations have been necessary, but he has been able to sleep regularly and work constantly, and at the present time he appears to be completely cured.

Case 3.—Minnie —, æt. 16. The lupoid ulceration first began when she was 12 years of age, unaccompanied by any pain. When she first came under treatment, the entire upper and lower lids and about one-third of the adjacent cheek were involved. Ether was administered, the entire ulcerated surface was curetted and the acid applied.

This case being at the Illinois Charitable Eye and Ear Infirmary, and my term of service about expiring, came under the charge of my colleague, Dr. Gardner, who continued the same course of treatment. After remaining a month or two in the institution, and no indication of the disease reappearing she was allowed to return to her home. Six months have now elapsed and there has been no return of the disease, but as there is slight ectropion of the lower lid, in consequence of the deep ulceration, a plastic operation will be necessary to restore it to its normal condition.

In all cases of lupus about the eye, I have considered it prudent to protect the cornea from the action of the acid, by means of a thick layer of absorbent cotton.

I have now used the pyrogallic acid in five cases, and in three or four very extensively and freely, yet in no case have I seen any symptoms which would indicate poisoning.

MEDICAL PROGRESS.

SEROUS CYSTS OF THE ORBIT.—At the meeting of the Académie de Médecine on December 14, M. PANAS read a communication on "The Pathogenesis of so-called Serous Cysts of the Orbit." The greatest obscurity has hitherto attached to the nature and pathological signification of these cysts. After a careful consideration of the subject M. Panas draws the following conclusions:

1. These cysts, without exception, are of congenital origin, even when their presence is not manifested until an advanced age.

2. They present seats of election, corresponding with the bronchial clefts of the embryo.

3. Their contents (epithelium, hairs, teeth, solid or liquid fat) may vary without the dermoid origin being doubtful, and this is due to the histological constitution of their walls.

M. Panas gives the record of an interesting case, with a full histological account of the cyst.—*Bulletin de l'Académie de Médecine*, No. 50, 1886.

DOUBLE PNEUMONIA TREATED WITH PILOCARPINE.—A man æt. 18, who had been greatly broken down with dysentery complicated with nephritis, suddenly acquired a double pneumonia. Antipyretics, expectorants, etc., resulted simply in a fresh lighting up of the dysentery. As in this way the organs of secretion as well as those that regenerate the blood became unfit for their work, uræmia and asphyxia combined threatened to set in, and to combat at least one of these dangers, recourse was had daily to hypodermatic injections to 0.1 gm. of pilocarpine. Thanks to the profuse perspirations and the salivation, purification of the blood was in this wise obtained until the lungs were again able to do their work. Improvement became more pronounced after every injection, and patient recovered in a very short time. With like good effect this drug was used in the case of an old lady sick with broncho-pneumonia, with intense dyspnœa.—*Memorabilien*, Hft. 4, 1886.

TREATMENT OF WHOOPING-COUGH.—The following method of disinfection of sleeping and dwelling apartments and clothes is recommended by M. MOHN in the treatment of whooping-cough. It is said to cure the cases immediately. The children are washed and clothed in clean articles of dress and removed to another part of the town. The bedroom and sitting-room or nursery are then hermetically sealed; all the bedding, playthings, and other articles that cannot be washed are exposed freely in the room, in which sulphur is burnt in the proportion of 25 grammes to the cubic metre of space. The room remains thus charged with sulphurous acid for five hours, and is then freely ventilated. The children return the same day, and may sleep and play in the disinfected rooms.—*Lancet*, Dec. 18, 1886.

STERILITY FROM TEA-DRINKING.—DR. DAVIS (*Therap. Gaz.*) states that tea-drinking acts in the direction of producing sterility in females.

THE
Journal of the American Medical Association.
PUBLISHED WEEKLY.

THE EDITOR OF THIS JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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SATURDAY, FEBRUARY 12, 1887.

PROGRESS OF MEDICINE AND VOLUNTARY MEDICAL ORGANIZATION UNDER A FREE GOVERNMENT.

The brief message of President Cleveland recently calling the attention of Congress to the propriety of making preparations for properly celebrating the completion of the first century of our country's progress under our present written Constitution establishing a true representative form of government, both State and National, suggested the question, What progress has medicine and medical organizations made under such form of government during the same period?

The written Constitution of the United States by which the previously existing thirteen loosely confederated States became one Nation, was passed by a Congress or Convention of delegates chosen for that purpose by the several States, and was officially promulgated on the 28th day of September, 1787. Consequently the 28th of September, 1887, will mark the completion of the first century of National progress under a well-defined constitutional form of government, adopted by the unrestricted choice of the people, and administered in all its departments by representatives chosen either directly or indirectly by the same people. Measures will undoubtedly be instituted for appropriately celebrating so important an anniversary day in our country's history; and if so it must provide for a careful and impartial review of our progress, not only in population, commerce, wealth, and political freedom, but equally so in education in its broadest sense, popular, scientific, and professional. It is perhaps the first time in the history of the human race, that an experiment on so grand a scale, unbroken for a century, has been allowed for determining how far a people holding not only all political power in their own hands, but equally the shaping and maintenance of all educational institutions, from the grammar school to the University with its departments of science, art, law, medicine and theology. In no department would such a review be more intensely interesting and profitable than that of medicine. Commencing the century in 1787 with but one organized medical college in actual operation in the entire country, and that one patronized by much less than one hundred students and granting less than a dozen diplomas annually; with State Medical Societies existing only in New Jersey and Massachusetts, and not a single medical periodical issued from the printing press of the country, and ending in 1887 with more than 100 organized medical colleges, educating more than 10,000 students and granting 3,000 or 4,000 medical diplomas annually; with active State Medical Societies in every State and inhabited Territory, all represented in one National Association, constituting a Republic of Medicine; while one hundred medical periodicals, weekly, monthly, quarterly and annual, are regularly issuing from the printing presses in all parts of the country, what a field for philosophical study is afforded by such a professional development, in comparison with all other elements of progress, all resting on the voluntary support of a free people.

It has been suggested that the attention of all the County and State Societies should be directed to a careful collection of all items of educational, scientific, sanitary and practical interest in their respective counties and States, and have it in readiness for such demonstration of the real progress of medicine during a century almost untrammeled by legislation, and equally unsupported by Government appropriations. The fact that the Ninth International Medical Congress is to be held in Washington, during the same month that marks the centennial of our constitutional government, gives much additional interest to the medical review we have suggested.

THE VEGETABLE ASTRINGENTS.

A valuable contribution to our knowledge of the vegetable astringents is a report to the Scientific Grant's Committee of the British Medical Association, by DR. RALPH STOCKMAN, of Edinburgh, on "The Action and Therapeutic Value of Vegetable Astringents." Dr. Stockman's investigation was undertaken with the purpose of determining the action of the vegetable astringents, after absorption into the blood, and of ascertaining to what extent

the current ideas as regards their therapeutic value are supported by experimental research. Dr. Stockman's paper may be found in the *British Medical Journal* of December 4, 1886. Briefly, the term "astringents," including "styptics," may be defined as "substances," which, either when locally applied, or after absorption into the blood, cause contraction of tissues, diminished secretion, and arrest hæmorrhage;" and those which act in this manner after absorption into the blood are known as "remote astringents." All the vegetable astringents which now have a reputation as remedial agents owe their value to tannic acid, but the chemical composition of this acid is not identical in all of them, the variety of tannic acid present being designated by such terms as "kinotannic," "catechutannic," etc. But all have the common property of precipitating albumen and gelatin from watery solutions, of producing an "astringent taste in the mouth, and of giving green or blue color reactions with persalts of iron.' The different varieties, however, have different powers of precipitating albumen. Gallic acid, which may be more properly classed as a remote astringent, does not precipitate albumen or gelatin, and does not tan; and its astringent taste is but barely perceptible Pyrogallic acid, while it precipitates albumen, differs markedly in other respects from tannin. In making his investigations Dr. Stockman selected, as representative members of the vegetable astringents, gallotannic, catechutannic, rhatany-tannic, gallic and pyrogallic acids, using the pure material, or as nearly pure as it could be obtained, first giving attention to the form and amount in which these bodies are absorbed, in which they circulate in the blood, and are excreted, and then to their action on the blood-vessels and circulation.

The experiments on absorption and excretion were made with gallotannic (ordinary tannic) and gallic acids. When tannic acid, says the writer, is taken into the stomach, it unites with the alkalies and the albumen present, and both these, and in the intestines, becomes converted, wholly or partially, according to the dose and time allowed, into gallic acid—as was shown by Schroff in 1853. "Absorption into the blood may take place as tannate and gallate of alkalies, or possibly as tannate of albumen dissolved in the alkaline intestinal secretion." Cavarra, Mitscherlich and Hennig, who investigated this subject over thirty years ago, found only a trace of these substances in the blood; and it is probable that their failure may have been due to crude processes of analysis. Dr. Stockman made ten experiments on rabbits to ascertain the form and amount in which tannic, or its derivatives, circulate in the blood. Tannic acid, in doses varying from one to ten grammes, dissolved in water, was given by the mouth, and the rabbits killed by bleeding at various periods after the administration of the drug; but not until it was ascertained by examination of the expressed urine that the addition of ferric chloride gave a copious blue precipitate. In no case did the blood or serum show the faintest blue coloration when ferric chloride solution was mixed with it on a white porcelain slab. The ordinary method of examining serous fluids was followed in the analysis of the blood. "The freshly defibrinated blood was precipitated with alcohol, filtered, and the residue washed with hot alcohol-ether and water." The filtrate and washings contained only the merest trace of a substance striking a greenish color with persalts of iron. Various other methods were tried, and with no better success; and it therefore seems evident that tannic and gallic acids are absorbed slowly and in small amounts from the intestines, and are so rapidly excreted from the blood that only a very small quantity is present in the circulation at any one time; and the experiments also show that tannic and gallic acids are excreted chiefly by the bowels, to a small extent by the kidneys, and by no other channel. Careful examinations of the various organs and secretions showed that only the urine, genito-urinary apparatus and the alimentary canal gave any reactions at all.

The only remaining method left by which to determine the forms in which tannic acid is absorbed, was by examination of the urine. Dr. Stockman confirms Lewin's observation that the amount of tannic acid excreted in the urine of rabbits is always considerable. Gallic acid is also invariably found, both being in combination with alkalies. The details of his method of analysis may be interesting. "The urine was distilled to a scrapy consistence either *in vacuo*, at a temperature of 110° to 120° F., or in a carbonic acid atmosphere. The residue was shaken up with alcohol, the whole filtered, the filtrate evaporated to dryness, and extracted with acetic ether. The acetic ether was then drawn off at a gentle temperature, and the residue (which must be quite free from water) washed several times with hot benzole. The washing with benzole was for the purpose of removing any pyrogallic acid which might be present, tannic and gallic acids being quite insoluble in it. The gallic acid was then removed by washing with anhydrous pure ether, in which tannic acid is insoluble. The residue left is dark-brown in color, syrupy in consistence, and contains the tannin

mixed with various impurities. When dissolved in water, if tannin be present, it precipitates albumen and gelatin (the alkali-tannate being decomposed by the acetic ether), the precipitate being soluble in lactic acid and alkaline carbonates. It also gives a copious blue-black precipitate with ferric salts, not clearing up on boiling." This method has the advantage that it is carried out at a low temperature, and oxidation is thus in a great measure prevented. From experiments on dogs it was found that when pure uncombined tannic acid was given there was generally found in the urine only gallic acid, with sometimes a varying, but always small, quantity of tannic acid. Whatever pyrogallic acid may be present cannot be detected by ordinary test for it. But if tannate of soda be given a large quantity of tannic acid will be found in the urine, along with a smaller quantity of gallic acid, pyrogallic being also absent in this case. Dr. Stockman's explanation of this is as follows: When uncombined tannin reaches the stomach (the contents of which are acid), it combines with the albuminous matters present to form tannate of albumen, only a minimal quantity combining with alkalies to form alkali-tannate. In the alkaline intestine, only such of it as has escaped combination with albumen is free to form alkali-tannates. The tannate of albumen thus formed is absorbed from the intestine with difficulty or not at all, and hence, remains there a comparatively long time, thus affording ample opportunities for the conversion of the tannic into gallic acid, in which latter form it is absorbed; the greater part being, however, excreted by the bowel. When, on the contrary, tannate of soda reaches the stomach, the chemical affinities of the tannic acid are already satisfied, and it is rapidly absorbed into the blood, thereby affording much less time for its sojourn in the intestine, and its conversion into gallic acid. Hence the large amount of tannic acid and the relatively smaller amount of gallic acid in the urine. The observations on man were conducted in a similar manner, and gave exactly similar results. Tannic acid may be given to man in considerable quantities and for a long time, without obtaining any bluish coloration on the addition of ferric salts to the urine. The fact that pyrogallic acid was not obtained in any case seems to show that it is not formed in the body.

Experiments with gallic acid showed that it has a dilating action on the blood-vessels; and that it has the power of dilating them again after they have been made to contract by the circulation of a weak alkaline solution through the frog. As alkalies contract vessels and gallic acid dilates them, we would naturally suppose that neutral gallate of soda would have no effect on their calibre; and a large number of experiments showed this to be the case. The circulation of salt solution containing gallic acid through mammalian blood-vessels also causes them to dilate. But when gallic acid is added to blood it becomes neutralized, and if the blood be circulated through an organ, either slight dilatation or no effect at all is produced on the calibre of the vessels. In the experiments the dilatation was in all cases so slight that it might be attributed either to accident, or, more probably, to the addition of the gallic acid diminishing the alkalinity of the blood. Though it has been generally assumed that tannic acid contracts blood-vessels, the experiments made by Dr. Stockman show that dilute solutions act exactly like dilute acids, and cause vascular dilatation. Solutions of catechutannic and rhatany-tannic acids, made with saline of the strength of 1:7500 to 1:1500, caused distinct dilatation. With such solutions it is sometimes possible to recover vessels which have been contracted by the circulation through them of alkaline solutions. From the fact that gallotannic acid is insoluble in sodium chloride solution it is difficult to investigate its action; for when it is added to the saline it is immediately precipitated, and in the circulation solid particles come in contact with the vessel-walls, thus causing contraction at once by coagulating the albumen. Still, in spite of the difficulties attending the experiments with it, it is proper to draw the conclusion that its action is similar to that of other varieties of tannin; and experiments on the excised sheep's kidney show that when the chemical affinities of tannin are satisfied it has no action on the vessels. Dr. Stockman's experiments with hypodermatic injections of tannic acid showed that it does not produce the effect stated by Fikentscher—that hypodermatic injections cause contraction of the arteries from stimulation of the vaso-motor centre in the medulla.

What, then, is to be said of the therapeutic value of vegetable astringents? In regard to gallic acid; it has no other action than that of a weak inorganic acid, either locally or when absorbed, and cannot be said to have any special action as an astringent. It does not lessen the calibre of vessels, either by peripheral or central action; and the fact that it does not precipitate albumen when used locally shows that it has no influence on catarrhal inflammation. It does diminish the alkalinity of the blood, and thus increases its tendency to coagulate; but stronger acids act in the same way, and more powerfully. "As regards tannic acid, the matter is somewhat more

complicated. Locally applied, its action for good depends on its power of precipitating albumen, the layer of tannate of albumen which is formed acting as a protective to the underlying mucous membrane. To this action is due its value in catarrhal inflammations of the alimentary canal, and in discharging surfaces generally. Its usefulness is limited to such cases, and as a remote astringent it is valueless. In weak solution, and uncombined, its action on contractile tissues, such as the vascular walls, is simply that of a dilute acid, while it is only when stronger solutions are used (much stronger than can ever exist in the blood, even if it were not in combination there) that its power of precipitating albumen comes into play. When its chemical affinities have been satisfied it is no longer capable of precipitating albumen, and therefore exercises little or no influence on the parts with which it comes in contact." The very small quantity in which it can exist in the blood at any one time also precludes its having any marked remote action; and as it is excreted by the bowels and kidneys, it can scarcely exert any effect on other mucous membranes, as the bronchial. Finally, while it is conceivable that it may have some influence in albuminuria, this is to be regarded as very doubtful; and the reported cases in which it has been used and careful measurements and analyses made confirm this doubt.

W. C. WILE, M.D., Editor of the *New England Medical Monthly*, has been appointed to a Professorship in the Medico-Chirurgical College of Philadelphia, and makes his future address 1006 Walnut Street, in that city, from which he will continue to edit his journal, as heretofore. Dr. Wile is one of the most active and talented members of the New England profession, and the Medico-Chirurgical College, though the youngest of the regular medical schools of Philadelphia, is nevertheless organized on a full graded system of instruction, and manned by a Faculty as worthy of the confidence and patronage of the profession as are her older competitors.

THE INTERNATIONAL MEDICAL CONGRESS OF 1887 AND TRANSATLANTIC STEAMSHIP FARES.—On the last page of THE JOURNAL of February 5 we gave a card from the Chairman of the Committee of Arrangements, Dr. A. Y. P. Garnett, giving information concerning liberal arrangements with several of the steamship lines, and the following appropriate acknowledgment should have been appended to the card: "The Committee wish to express their thanks to Mr. Edward Droop, of Washington, for his active interest and efficient aid in consummating this arrangement."

SOCIETY PROCEEDINGS.

MEDICAL SOCIETY OF THE DISTRICT OF COLUMBIA.

Stated Meeting, January 12, 1887.

THE PRESIDENT, JOS. TABER JOHNSON, M.D., IN THE CHAIR.

H. M. CUTTS, M.D., Secretary.

DR. D. S. LAMB presented

A PORTION OF THE ILEUM, WITH PERFORATION, FROM A CASE OF TYPHOID FEVER.

This specimen consists of a portion of ileum removed about 1½ feet from ileo-colic valve. It contains several Peyer's patches, all of which show slight thickening. The lowermost one is thickened, ulcerated and perforated. The ulcer is irregular in outline, edges slightly undermined; the base shows muscular fibres. The perforation is one-eighth inch in diameter; peritoneal surface of specimen plastered with pseudo-membrane. The case is one of typhoid, or perhaps more properly *enteric*, fever. The patient was a white man, age 29; had been perfectly healthy until this sickness. After several days malaise, typhoid fever was diagnosticated. He died ten days afterwards. The disease appeared in all to last two weeks. Temperature, taken twice daily, ranged from 102° to 104°. Symptoms of perforation occurred the day before death. The attending physician, for whom I made the necropsy, was unable to arrive at the cause of the disease. The post-mortem examination showed the body well nourished, lungs and heart normal. There was dirty, malodorous liquid in abdomen; some flatulent distension of stomach and intestines; intestinal peritoneum reddened, and in right iliac region plastered with dirty looking lymph. In lower two feet of ileum some Peyer's patches were nearly normal, others thickened and irregularly ulcerated; and one of them ulcerated and perforated, as described. Small ulcers in cæcum and vermiform appendix; liver normal; gall bladder ulcerated and contained over eighty gall-stones with facettes; spleen large and soft; kidneys normal.

According to Murchison, quoted by Fagge, peritonitis occurs in three per cent. of all cases of typhoid fever, and in nearly twenty per cent. of all deaths from the fever. The cause of the peritonitis is usually, as in this case, a perforation of the ulcerated Peyer's patch and escape of the intestinal contents into the peritoneal cavity. The perforation is most often excited probably by a sudden movement of the patient, as vomiting, straining, rising up; or by flatulent distension of the bowel, enemas, etc. The indication is, of course, to prevent so grave an occurrence by insuring the utmost quietude to the patient. And since perforation occurs sometimes in cases called mild, the patient perhaps walking about, the necessity of absolute rest is the more emphatic. In this particular case the date of perforation is placed at the end of the second week, which is several days earlier than that usually assigned. When,

however, it is considered how difficult it is to ascertain the precise date of beginning of the disease, the actual date of perforation becomes doubtful. The fact that many Peyer's patches in the lower ileum were but little thickened, while scattered among these were others ulcerated and one perforated, seems to confirm the theory that usually all the patches are not involved at one time, but with some hours or days of interval between. The fever, depression and danger are doubtless greater when all the patches are involved at the same time. But few cases recover after perforation in typhoid fever. It is quite likely that laparotomy has been tried in some cases, but I have not looked this up, and from what we know of the disease I should not expect healing to take place. I regret that the attending physician has not found it convenient to furnish a history of the case.

Dr. S. C. Busey remarked that as Dr. Lamb had brought up the question of laparotomy in perforation of typhoid fever, he would like to hear the subject discussed. Neither Dr. Lamb nor himself could recall an operation with this in view. As he understood Dr. Lamb, he said that the physician attending the case could not make out the cause of the fever. There is no question but that there is a specific cause for typhoid fever. Usually we get it in drinking water, food, etc. While it is easy to diagnosticate typhoid, it is not always so easy to tell when perforation has taken place. The peritonitis, which is sometimes present in typhoid, cannot always be ascribed to perforation. He has seen cases complicated with peritonitis get well, but he was sure that there was no perforation, because such cases usually die. Cases of recovery after perforation have been reported, but the diagnosis was only presumptive.

In his opinion, if the diagnosis is certain, laparotomy is justifiable, as the only hope of saving life. Of course, the diseased Peyer's patches and extravasation of fæces would complicate matters a great deal. He thought it difficult to make a diagnosis of perforation. He said that he used to think that all cases of peritonitis were from perforation, but now he believes that if they get well there has been no perforation.

Dr. J. Ford Thompson said that he had had no experience in laparotomies for perforation in typhoid. He thinks that while the operation may be justifiable, it would, probably, be unfavorable, not only on account of the local condition of the bowel, but on account of the general condition of the patient as well. He does not think that union of the gut would take place readily even if it were healthy. A better procedure would be to make an artificial anus, with the intention of operating again when the patient had somewhat recovered. He thinks the diagnosis of perforation easily made. The wound cannot be closed as a bullet wound, for if it was it would probably tear open in a few days and we would be worse than when we started. He thinks that the artificial anus will eventually be the legitimate operation.

Dr. H. D. Fry said that the specimen illustrated one reason why repair of perforation would be difficult. There was so much diseased tissue about the perforation, which it would be necessary to take away, that, as Dr. Cook has just suggested to him, cicatricial stricture would be likely to follow.

Dr. Lamb explained to Dr. Busey that the physician could not find the cause of this particular case of typhoid, not that the specific germ of the disease was not known.

Dr. R. Reyburn thinks that laparotomy for typhoid perforation has been done, but he cannot now recall where he has seen the report. He thinks the diagnosis of perforation easy, and that laparotomy would be justifiable.

Dr. Jos. Taber Johnson read a paper entitled

CAN THE CÆSAREAN SECTION SAFELY SUPPLANT CRANIOTOMY IN THE UNITED STATES AT THE PRESENT DAY?

(See page 169.)

Dr. A. F. A. King congratulated Dr. Johnson upon the masterly manner in which he handled the points of his argument, and upon the mode of presenting his figures. Two years ago, he said, he read a paper giving some conditions in which craniotomy would be absolutely necessary, and now, if after hearing them once, he had understood them correctly, he endorses Dr. Johnson's views and declares his belief that craniotomy will always prove a valuable operation.

Dr. J. Ford Thompson said that he had been following Dr. Johnson's paper very closely, and as near as he could make out the doctor recommended craniotomy in this country and Cæsarean section in Germany, where there are better operators. Dr. Johnson even expresses his personal preference for Cæsarean section, but advises us to do craniotomy. This cannot be the scientific way of looking at the question. One or the other is right, and the duty of the scientific surgeon is to find out the truth as to which is the better operation. There must be something better than the ghastly operation of craniotomy. There can be no doubt, however, but that in the hands of the ignorant more mothers will be saved by it than by Cæsarean section; but the ignorance of the operator is not the question to be considered. Other things being equal, which operation will save more lives, is what we must endeavor to find out.

If he had a good case and was called upon to decide on the operation, he would choose the Cæsarean section. He would not select the Sänger operation in all cases. Sänger's operation has a wonderful list of successes following it, but on the other hand he has seen C. Braun save three mothers and three children by the Porro. It is not so much the operation, as the time at which it is done. It is like the operation for strangulated hernia. The operator is hardly responsible for a fatal issue when the patient is brought to him after a prolonged taxis. If done in time the result would almost certainly have been successful. Thus, no one system can be implicitly followed out, and he would advise the Porro where the pelvis is so small that a future pregnancy would be likely to be fatal, and a Sänger where there was a possibility of a subsequent premature delivery.

Craniotomy may also be useful. If he found the patient very far gone and the fœtus dead, he would

advise craniotomy. In any case, if the mother alone is to be regarded, craniotomy is the safer operation; but if the child be alive, and we are to consider how most lives are to be saved, Cæsarean section is the operation.

Upon Dr. King's calling the speaker's attention to the fact that craniotomy is more fatal to the mother than Cæsarean section, when the pelvic diameters are below 1½ inches, Dr. Thompson said he supposed that that exception was understood.

DR. R. F. A. KING said he had long been in doubt as to whether the argument advanced by Dr. Barnes, viz.: that of considering the number of human lives that might arise from future pregnancies if the woman were saved, was a legitimate one. Should we admit this argument, on the one side, by saying that if the woman survive she might bear other children by induced premature delivery; so, on the other hand, it might be said that if she did *not* survive, her husband would then be at liberty to find a better breeder in another wife. Moreover, if the question of increasing population is to be admitted, we should also have to consider the future capacity of the child, or children, in this respect. But he thinks it is not a matter so much of *populational increase* as it is one mostly, or entirely, applying to the *individual* life of the lying-in woman and the *individual* child to be delivered. We deal with the existing emergency only, or at least chiefly: future children, yet unbegotten, must be left for the future.

DR. H. D. FRY said that he thought some explanation of Dr. Garrigues's position in regard to the Sänger operation was due to that gentleman. It did not seem to him that Dr. Garrigues, as implied in Dr. Johnson's paper, meant to claim for himself the origin of the so-called Sänger modfication. He raised the question whether an operation should be named after the one who proposes it or after the one who first performs it. Dr. Garrigues thought it might with equal justice be called after his name because, in a published list of the number of Sänger's operations performed, he occupied third place and Sänger eighth. His operation therefore antedated Sänger's. The method, moreover, differs as now practised from the original one suggested by Sänger. It has been found unnecessary to remove the wedge-shaped piece of uterine tissue. He said, further, he was not familiar with the histories of the five cases operated upon in this country after the Sänger modification, all of which, as Dr. Johnson states, terminated fatally. In the case operated upon by Garrigues, the patient had phthisis and was in the worst possible condition to stand any grave surgical operation. At the autopsy the peritoneal wound was found united and the uterine cavity entirely separated from that of the peritoneal. In this instance it is unfair to attribute the result of the operation to a failure of the Sänger modification.

Dr. King very properly makes a point of the degree of pelvic contraction existing in a given case when considering the advisability or not of craniotomy. The statistics of the operation in this country show a mortality of 40 per cent. in the higher degrees of contraction. Consequently our surgeons make as bad a showing, compared with Continental operators, in craniotomy as they do in Cæsarean section. In Europe craniotomy gives a mortality of only 8 per cent. or 5 per cent, and some operators have had a series of cases without a single death. There are three principal causes, Dr. Fry thinks, why our success in this line is not so good as that of our foreign brothers viz.: They follow antiseptic precautions more rigidly; they have had greater experience with the improved modifications of Cæsarean section; and they possess a better knowledge of pelvimetry—and, Dr. King added, more cases of pelvic deformity. Two of these disadvantages, the doctor continued, the surgeons of this country should overcome; the third is more difficult to remove, because the facilities for obtaining equal knowledge in pelvimetry do not exist in our country.

The importance of being able to ascertain the degree and nature of the pelvic deformity is indicated thus:

Suppose, in a case of dystocia, the antero-posterior diameter of the brim is ascertained to be three inches. With this amount of contraction of the conjugata and with no knowledge of the transverse and oblique diameters, efforts may be made to deliver by version, symphysiotomy or craniotomy. If the case, however, should be one of justeminor or equally faulty pelvis and a decrease of one inch in all the diameters, the early performance of Cæsarean section would offer the best chances for the mother, and efforts at delivery in other ways would jeopardize the result.

The great value of enforcing antiseptic precautions is shown by the fact that before the introduction of this method, Cæsarean section was uniformly fatal when performed by skilful operators connected with maternity hospitals. Now, they obtain brilliant results by the use of antiseptics. The German operators take the most minute precautions to avoid infection. Let the surgeons of this country advance in this respect. They can do that and they can operate skilfully, and adopt all the improved modifications of performing Cæsarean section.

DR. S. C. BUSEY said that he would like to congratulate Drs. King and Johnson on the reformation of their opinions. Two years ago, when he had advocated entirely abolishing craniotomy, these gentlemen had opposed his ideas.

(DR. KING explained that he had not altered his views. He had opposed the complete doing away with craniotomy, which Dr. Busey had advocated, and had himself shown conditions in which it would be absolutely necessary.) Dr. Busey replied, that at all events he had made some progress, as he had just indorsed Dr. Johnson's paper.

DR. BUSEY, continuing, said that he congratulated himself not a little that the views expressed by him two years ago were coming to be held by the profession at large. If he recalls Dr. Johnson's paper rightly, the chief reason why craniotomy should still be practised is that it is an easier operation than Cæsarean section—that there are still so many men illtaught that it cannot be entirely done away with. This cannot be, as Dr. Johnson says, the scientific standpoint from which to look at the question.

The doctor quotes from a table of Sänger operations prepared by Dr. Harris (*Med. News*, September 18, 1886), in which 38 cases are reported. Of the 76 lives involved 59 had been saved. He compared these with 38 cases of craniotomy, where at the outset it is only possible to save the 38 mothers, and called attention to Dr. King's remark that in the high grades of contracted pelves craniotomy was more fatal to the mother than Cæsarean section, thus making the outlook of saving even the 38 mothers a bad one.

Dr. King remarked that he did not think that a good basis of comparison.

Dr. Busey replied that the two operations were always to be compared. Each was the alternative of the other, and no woman was to be delivered by craniotomy or Cæsarean section who could be delivered by forceps, version or symphysiotomy.

Dr. Johnson thought that 38 cases of Sänger's operation was rather more than had been done, and put in evidence an autograph letter from Dr. Harris giving the synopsis of only 26 cases collected by the writer, and he further reminded Dr. Busey that his paper dealt only with the Cæsarean section in the United States.

Dr. Busey replied that the discussion of such a question should not be limited by any boundaries. Even if we confine ourselves to the United States, Dr. Johnson has wrongly undertaken to lay down a law by previous unlucky experiences, and to assert that the results will continue bad, in the face of the fact that surgery has made the most astonishing advances in the past few years, and is capable of advancing still further. By limiting the discussion to one locality he is discouraging the surgeons of that locality from making advances, and just so long as craniotomy is taught, it will be practised, because there will be men too ignorant to do Cæsarean section. The more craniotomies there are done, the more there will be done. He agreed with Dr. Meadows that it will have to be discussed from a moral as well as from a scientific standpoint, for just so long as teachers teach to kill at will the killing will be done.

Dr. King remarked that the killing could hardly be said to be done at will.

Dr. Busey replied that it was most assuredly done at will. The operator fully considered the mother before proceeding, and then voluntarily and at will killed the child.

Dr. Acker asked Dr. Busey how he would get around the objections of the friends and relatives to the Cæsarean section?

Dr. Busey replied that their objections did not relieve the doctor of his responsibilities—that because they will not permit the right thing to be done, it does not make it right to do a wrong act.

Michigan State Medical Society.—The executive committee have changed the date of the annual meeting of the Society to May 12 and 13, so as not to conflict with the meeting of the American Medical Association.

CHICAGO GYNÆCOLOGIAL SOCIETY.

Regular Meeting, Friday, December 17, 1886.

The President, Chas. Warrington Earle, M.D., in the Chair.

(*Continued from page 164.*)

ÆTIOLOGY, PATHOLOGY AND CLASSIFICATION OF SALPINGITIS.

The Secretary, Dr. Edward Warren Sawyer, read the following communication from Dr. Sänger, of Leipsic, in reply to a letter by Mr. Lawson Tait, read before the Society, May 28, 1886.

Dr. Sänger's letter was translated by Dr. Ivo Bernauer, of the Cook County Hospital, and revised by Dr. Christain Fenger.

Leipsic, Oct. 10, 1886, Lindenstrasse 16.

To Daniel T. Nelson, M.D., President of the Chicago Gynæcological Society.

Dear Sir: I have been personally attacked by Mr. Lawson Tait in a letter addressed to you; as I desire that my reply go by the same way, I take the liberty of requesting you to bring my letter to the notice of your Society and to have it assigned a place in the Transactions of the same. Nobody will dispute that up to the present time Mr. Lawson Tait, of all laparotomists, has had the best results, at all events in regard to ovariotomy and salpingo-oöphorectomy. His practical results have, however, raised his conceit to so high a degree that in pathological questions also he assumes a certain infallibility, which vents itself in numerous sallies and attacks upon others. The consequence of this is that just at present, Mr. Lawson Tait is being subjected to various energetic criticisms as by Bigelow, Schröder and others.

Now Mr. Lawson Tait has also shot one of his shafts at me. I feel very thankful toward Dr. Christain Fenger for having received it on my account. However I do not wish to allow the matter to rest there.

At the sesssions of the Gynæcological Society of Chicago, held in February and March, the treatment of pelvic abscess by laparotomy was discussed in a highly instructive manner. Dr. Fenger in his excellent and exhaustive remarks, said that "my statements regarding ætiology were the most complete" inasmuch as there must necessarily be as many forms of pelvic abscess as there are forms of disease of the Fallopian tubes. The latter were enumerated by Dr. Fenger, according to the classification given by me in a paper read at the *Versammlung Deutscher Naturforscher* in Magdeburg. (See report, Archiv. f. Gynæk. Bd. XXV, pp. 126–33.) In this classification six different forms are recognized, and it is this distinction which Lawson Tait is pleased to style "absurd." Although Dr. Fenger reiterated his statement "that he regarded my classification as correct and complete and in accordance with the laws governing inflammatory processes in all organs of the body," yet I do not think that this defense of my position is sufficient. Considering the great influ-

ence Lawson Tait exercises upon the profession, I deem it my duty to refute him in every particular. I shall attempt to do so in a scientific manner, and I may thus hope that my reply will prove of general use and interest.

The pathological anatomy and the course of salpingitis can be understood only when we bear in mind the theories of infection. The whole sexual tract, from the ring of the hymen to the *ostium tubæ abdominale*, is open to the entrance of the external air and the germs suspended in it. Carriers of infection coming from the abdominal cavity and its contained organs may also enter at the *ostium tubæ abdominale*. Even microbes originally lodged in the external parts, in the vagina around the cervix, may, by way of the lymphatics, reach the peritoneal cavity, and thence gain entrance into the tubes.

The normal vaginal and uterine secretions at the age of puberty and the menstrual blood contain numerous non-pathogenous micro-organisms. Still greater numbers are found in the catarrhal secretions of the uterus in cases of endometritis as was demonstrated by Küstner. As to the normal tubal secretion and the tubal secretion in cases of *salpingitis catarrhalis*, consequent upon *endometritis catarrhalis*, no investigations have as yet been made to show whether or not they likewise, contain non-pathogenous microbes. However, as the secretion of an endometritis contains microbes we may assume that if the inflammation is continued into the tubes, its secretion will here likewise contain the same. It has been clearly proven that pathogenous micro-organisms pass from the external parts to the tubes, and the peritoneal cavity, a fact which is doubted by no one, perhaps, except by Lawson Tait. These organisms have, in part, been acurately studied, and it is well known that different kinds produce distinct forms of salpingitis, and secondarily pelveo-peritonitis. The fact was already established by Guréin and Guerrier, that in making vaginal and intra-uterine injections air, and thus, also micro-organisms, might pass into the tubes (Physo-Salpinx). S. Hennig, in his book "*Krankheiten der Eileiter*," p. 52, surmises that, in cases of putrid endometritis and physometra, gases may escape from the tubes into the peritoneal cavity.

Our present knowledge of the above mentioned pathogenous micro-organisms will enable us to divide them into three groups:

GROUP I.—FORMS OF SALPINGITIS PRODUCED BY KNOWN SPECIFIC MICROBES.

1. *Salpingitis gonorrhoica*, produced by the gonococcus of Neisser.

2. *Salpingitis tuberculosa*, produced by the *bacillus tuberculosis* of Koch.

3. *Salpingitis actinomycotica*, produced by the *actinomyces bovis* of Bollinger.

1. *Salpingitis gonorrhoica* in the only specific infectious form of salpingitis which is recognized as such by Lawson Tait, although he stops short of admitting that the gonococcus is the exciting agent. Without doubt the gonorrhœal is the form most frequently met with. This fact was clinically established as early as 1872, by Nöggerath, long before Neisser had discovered his gonococci or Lawson Tait performed his first operations for "suppuration of the uterine appendages." In Germany, I myself was one of the first gynæcologists who at our meetings showed the frequency of gonorrhœal salpingitis, emphasized its causal connection with pelveo-peritonitis, and removed by operation the gravely implicated uterine adnexa. (Magdeburg, 1884, and Munich, 1886.) Gonorrhœal salpingitis is never followed by a destructive "suppuration" of the uterine appendages; it remains invariably a disease of the surfaces of the mucous and serous membranes. The pus formed by the specifically diseased mucous membrane gradually distends the tube; in one class of cases in which there is a great accumulation of free pus the tube is transformed into a large sac with thin walls, in another in which the wall of the tube, especially its muscular tissue, is hypertrophied to a greater extent, the tube becomes much thickened and rigid. In most cases, both conditions are found, the uterine portion of the tube is thickened, the abdominal end dilated. The serous surfaces of the tubes, the albuginea of the ovaries, the serosa of the peritoneum are attacked or become pus secreting surfaces only in cases in which gonorrhœal pus has escaped from the tubes, and thus infected the above named structures. We may then have peri-salpingitis, peri oöphoritis, peri-metritis, s. *pelveo peritonitis purulenta gonorrhoica*. I do not believe that gonorrhœal pus ever penetrates the walls of the tubes and thus produces these diseases. But a specific gonorrhœal inflammation of the mucous membrane of the tube, with secretion of pus into the cavity of the latter, is accompanied by a non-specific inflammation of the entire tubal wall. This may also excite peri salpingitis, peri-oöphoritis and so forth; the organs involved may become adherent to each other and displaced, but we never meet with a purulent exudate of the same nature as that found in the cavity of the tube itself. This also explains why, in some instances, gonorrhœal disease of the uterine appendages is accompanied by severe and violent symptoms, frequently resembling those of a peritonitis following perforation, whereas in other instances it develops insiduously, scarcely manifesting any symptoms at all. In the former cases, gonorrhœal pus escapes through the *ostium abdominale* into the peritoneal cavity; in the latter the inflammation of the external surfaces of the adnexa is non-specific in character.

According to what I have just stated, I must necessarily regard the terms, "suppuration of the uterine appendices and peri-uterine s. pelvic abscess." as inaccurate, and from a general pathological point of view, as productive of confusion; we invariably find free pus in the tubes and peritoneal cavity or an inflammation of the adnexa, but never destructive suppuration of the tissues of the pelvic organs. In cases in which abscesses are discovered in the walls of the tubes, in the tissues of the *ligamenta lata* and in the ovaries, these abscesses are, as I shall later on show, due to septic infection, but not to gonorrhœa.

The latter disease produces suppuration only on surfaces.

I purposely enlarged somewhat on gonorrhœal salpingitis and its consequences, as this form presents a typical example of infectious salpingitis in general.

There is one more point to which I wish to call attention. Gonococci have not always been discovered in pus coming from the tubes, in cases in which clinically there existed no doubt as to the gonorrhœal nature of the infection. The conditions under which the gonococci are destroyed, or prevented from further development, have not yet been ascertained; further investigation will also have to show whether, in cases in which gonococci are absent, there are not present other microbes belonging to one of the groups mentioned further on.

2. *Salpingitis tuberculosa.*—Alfred Hegar's lately published work, "*Enstehung, Diagnose und Chirurgische Behandlung der Genitaltuberculose des Weibes,*" relieves me of the necessity of entering more fully into the consideration of this form of salpingitis. Lawson Tait denies the existence of this form, or rather, he admits it, but only "for the third and contracting stage of pyosalpinx." This admission simply discloses his ignorance of the true nature of tuberculous infection. The pus in a case of purulent salpingitis, whether it be gonorrhœal or otherwise, may of course undergo caseation. This was called tuberculization before Koch's discovery of the *bacillus tuberculosis;* now it is termed coagulation-necrosis, according to Cohnheim-Weigert. It is this, which Lawson Tait confounds with the genuine infection by the bacillus of tuberculosis.

A pyosalpinx may remain in this third stage indefinitely; a tuberculous salpingitis will never result therefrom unless there be added a tuberculous infection.

3. *Salpingitis actinomycotica.*—This form is called by Lawson Tait "an equally ridiculous subdivision based on mere theory, not on fact." It seems to me before making such an unintelligible assertion it would have been his duty to enquire whether there really is no case on record to support me in including this form in my enumeration. In my paper, above mentioned, I named the author who had furnished this case; I will now accurately give my authority: Adolph Zemann, "*Uber die Aktinomycose des Bauchfells und der Baucheingeweide beim Menschen, Medicin. Jarhbücher der K. K. Gesellsch, Arzte in Wein,*" 1883, S. 477, Fall 4. The tubes in this case were dilated and filled with pus and clumps of the actinomyces, their walls were thickened and exhibited numerous granulations produced by the fungus. The fungus had migrated either from the vagina or from the intestines which were found extensively adherent to the tubes. What Lawson Tait does not know, has no existence for him. "Germanica sunt, non leguntur."

GROUP II.—FORMS OF SALPINGITIS DUE TO SPECIFIC MICROBES IDENTICAL WITH THOSE PRODUCING TRAUMATIC INFECTION.

Salpingitis septica (pyæmica, ichorosa, purulenta, diphtheritica).—The term *salpingitis septica,* is rather general and inaccurate; as when speaking of a pyosalpinx we simply mean that the tube contains pus, when employing the term *salpingitis septica* we merely indicate that the disease is due to infection by a septic virus. It is at the present time, a matter of extreme difficulty to diagnose the nature of the pus, and the nature of the infection presented to us in an individual case. Now, we certainly know that the microbes producing the different kinds of traumatic infection known clinically as septicæmia, pyæmia, diphtheria, phlegmon, erysipelas, may one and all invade the general tract; we may, hence, infer the existence of an equal number of varieties of salpingitis, *i. e. salpingitis septica, pyæmica, diphtheritica, phlegmonosa, erysipelatosa.* In order to complete our scheme we should add *salpingitis putrida,* corresponding to putrid infection, whereby the difficulties are still further increased.

Notwithstanding the progress made in bacteriology, we have not yet succeeded in isolating and classifying the microbes which cause the clinically different forms of traumatic infection; consequently it is impossible to do this with regard to the different forms of *salpingitis septica.* However, the work done by Doléris, E. Fränkel, Lomer, A. H. Barbour, Nöggerath, Cushing, in the domain of puerperal infection has given us some positive results.

These are two points which are to be considered fundamental:

(1). The microbes of puerperal septicæmia are identical with those producing traumatic infection in general. During the puerperium after abortion, as well as after parturition at term, the genital track is far more susceptible to infection, or the conditions are far more favorable to the spreading of infection, than at other times.

(2). As has been demonstrated by Ogston, Hueter and Rosenbach, all suppuration is due to the action of microbes; several varieties of these, like the *streptococcus pyogenes* and *staphylococcus pyogenes,* have been closely studied, but it is known that they are not the only varieties which produce pus. As doubtless all of these carriers of infection may play a rôle in the production of salpingitis, we can readily see how complicated the question of infectious diseases of the tubes has become, and how unscientific and untenable is the meaningless name of pyosalpinx. Yet there is a certain comfort in hoping that the matter may be somewhat simplified. Some of the forms of traumatic infection, for instance, sepsis, and diphtheria, pyæmia and phlegmon, are probably produced by identical micro organisms, and the course of the disease may be modified by the nature of the tissue first attacked, and by the manner in which the infection spreads, whether by the blood or lymph channels.

All of these infections, as is well known, have a double effect—a local one in the genital tract, and a constitutional one, which is brought about through the medium of the circulation, and which is seen not only in the system at large, but also in the localization of the infection in organs distant from the point of entrance of the micro-organisms. When systemic effects are produced by the virus of putrid infection,

the disease is called sapræmia; when by the virus of septic infection, septic toxæmia or ptomaine poisoning; when by that of purulent infection, pyæmia.

We thus meet with an essential difference between the diseases of the genital tract, produced by the microbes of Group I, and those of the traumatic infection. The above mentioned severe and acute constitutional symptoms are but slightly indicated, or may even be absent in the diseases of the first group, *i. e.* gonorrhœa, tuberculosis, actinomycosis, whose course is chronic; whereas in diseases of the second group, the rapidity of their development, and the malignity of their course may almost entirely obscure the local disturbance in the genital tract.

As is readily seen, the septic diseases of the tubes are not independent diseases; aside from the local disturbances co existing in the uterus, vagina and external genitals, the whole circulatory system is usually affected. In this respect, as Lawson Tait rightly remarks, septic disease of the tubes is not a specific ailment; this, however, is of no importance in an enumeration of the varieties of salpingitis. An endometritis or a *colpitis gonorrhoica* may co-exist with a *salpingitis gonorrhoica*, and in the same way *salpingitis septica* may be accompanied by other diseases of the sexual organs, which are due to the same cause. There is, of course, no such thing as an affection of the tubes merely.

Salpingitis septica, co existing with severe puerperal septicæmia or "lymphatic peritonitis," has never as yet, it is true, given the surgeon an opportunity to remove the principal focus of the disease by extirpation of the tubes. It is possible, however, that under certain circumstances such a procedure might be indicated. B. S. Schultze, "*s. Verhandl. d. gynækolog. Section d. Versammlung deutscher Naturforscher*," in Berlin, 1886, has lately succeeded in amputating a puerperal uterus, in a case in which it was impossible otherwise to remove the placenta, which had become the source of septic infection. Lately two cases came to my knowledge in which the tubes burst from over-distension with pus, whose nature, whether gonorrhœal or septic, was not ascertained. In both cases a general peritonitis resulted, which proved fatal, in one on the fourth, in the other on the twenty-first day after confinement. It is clear that in both these cases the salpingitis had existed before delivery. I shall afterwards relate a case of my own in which this certainty was the condition. Cases of this kind will be diagnosed more frequently and more readily as soon as our attention has been called to them, and we may then expect to hear of their treatment by operation. Cases of salpingitis, consequent upon traumatic infection in non-puerperal women, are, of course, of much more frequent occurrence. The carriers of infection may, for instance, be directly introduced into the tubes by means of an infected sound. The introduction of a septic instrument into the uterine cavity may be followed by a septic salpingitis. In cases in which we observe an exudative pelveo-peritonitis after the introduction of an infecting sound, after an intra-uterine injection or a *curettement* of the *mucosa uteri*, the infection almost always spreads to the pelvic peritoneum by way of the tubes and exceptionally only through the muscular walls of the uterus. The severe systemic disturbances, the diseases of the uterus and pelvic peritoneum, may gradually subside, whereas the tubal affection remains. A pyosalpinx has formed, the tubes are filled with pus, which can have been produced only by the action of one of the specific microbes of traumatic infection: perhaps by the *staphylococcus pyogenes*.

Two cases of my own may serve as illustrations. In one of them a physician had performed *abraiso mucosæ uteri* without antiseptic precautions, infection took place and a *pelveo-peritonitis exudativa* followed, after the subsidence of which I could easily feel each tube thickened to the size of a thumb. At the operation both tubes were discovered to have thinned walls and to contain thick pus resembling that found in an abscess. The ovaries were small and enveloped in masses of very dense, connective tissue. I removed the tubes, but left the ovaries. The woman made a good recovery. The adnexa had been in a healthy condition before the mucous membrane of the uterus had been scraped. I have already published the other case (*"Verhandlungen der Gesellch. f. Geburtsh," zu Leipzig*, 17 April, 1882; *"Centralb. f. Gynæk."* 1882, p. 558); multipara of twenty-nine years; three spontaneous deliveries, at fourth *placenta prævia*, and forceps applied before complete dilatation of the os. Puerperal fever. Recovery, but permanent pains in the right epigastrium. A short time after, renewed pregnancy, in the third month of which a prominent gynæcologist ascertained disease of the right uterine appendages; at full term rapid and spontaneous delivery. On the third and subsequent days of the puerperium, chills, high intermitting fever, icterus, in short the symptoms of acute pyæmia. Death on the thirtieth day. The autopsy revealed *salpingitis purulenta dextra*, and several abscesses in the right ovary and right broad ligament. The remainder of the genital organs, the adnexa on the left side normal. I explain the course which the disease took in this case in the following way: At the fourth parturition, septic infection and localization of the disease in the uterus and the appendages on the right side; subsidence of the grave constitutional disturbances and persistence of a pyosalpinx, probably due to *streptoccocus pyogenes*, latency of pyosalpinx during subsequent pregnancy. After delivery increased absorption of pus, abscesses in the ovary and broad ligament, acute pyæmia and death. In a case of this kind a septic-infectious disease, originally extending over large portions of the sexual tract, is finally concentrated in the tubes. Why should not such a case, in which the tubes chiefly appear affected, present a "specific ailment" as well as does a case of *pyosalpinx gonorrhoica*, in which *ceteris paribus* we have the same limitation of the disease to the tube as a principal focus for further infection.

What I desire to prove by these somewhat extensive remarks is, briefly, as follows:

1. Numerous cases of *salpingitis purulenta* ("pyosalpinx") are due to traumatic infection, are septic forms of salpingitis.

2. There are as many forms of septic salpingitis as there are forms of traumatic infection, and of microbes producing the same.

There is, however, an additional reason why Lawson Tait denies septic salpingitis to be a specific ailment. As we see from his startling remarks in the *Medical News*, April 24, 1886, he does not believe in sepsis at all, does not believe in infection, denies the principles on which the practice of modern surgery and obstetrics is based. He has been taught nothing by the researches of Semmelweis and Lister, Pasteur and Koch. And why? Because his own success in combating septic infection is to him proof of the non existence of septic infection. We are accustomed to Lawson Tait's reckless statements. Without taking the trouble to refute them scientifically, I but wish to call to mind his assertion that menstruation does not depend upon the ovaries, but upon the tubes; that the mortality of the Cæsarean operation is still 99⅔ per cent. His denial of traumatic infection is a statement of the same kind. I should like to hear Lawson Tait's answer to the following questions:

1. What disease, before the introduction of Listerism, killed trousands of patients who had received wounds or who had been operated upon? What, according to his views, was the cause of death in his own cases, when he lost patients after operations, if not, as everybody else believes, septic infection?

2. What is puerperal fever?

Lawson Tait disputes the correctness of our teachings regarding infection, but he has failed to give us any other explanation of its phenomena.

(*To be concluded.*)

DOMESTIC CORRESPONDENCE

"ELECTROLYSIS IN UTERINE FIBROIDS."

Dear Sir:—In Dr. Cutter's letter of last week, "Electrolysis in Uterine Fibroids," I am indirectly criticised for remarks that I wish to modify somewhat as far as they concern me. Dr. Belfield's remarks on my article, "Electrolysis in Gynecology, with a Description of Dr. Apostoli's Method," were quoted by Dr. Cutter, and these remarks made me appear to say, "During the last twenty-five years various attempts have been made to reduce fibroids of the uterus by the galvanic current; yet none of them have been recognized as successful; because, doubtless, as Dr. Martin very properly says, the current has been used in an ignorant, inaccurate and bungling way." I do not wish to be understood as saying, "yet none of them have been recognized as being successful," because I have reported successful cases within the last two years of my own, and have also quoted successful cases of other operators. Neither do I wish to be quoted as saying, "the current has been used in an ignorant, inaccurate and bungling way," because it is not what I said, although I do not impute to the author of the remark any intention to misquote. What I *did* say, however, was this: "Unfortunately, the term electrolysis, as applied to the treatment of diseases, has been by many, for some unaccountable reason, greatly misunderstood." This statement, while it does not differ widely from the remark quoted, is somewhat modified and not nearly as sweeping.

Dr. Cutter, I am sure, will agree with me, after having had sixteen years' experience with this method of treatment, that there is a great deal of ignorance abroad in regard to the principles it involves. He must also admit that improvements in the methods of electrolytic treatmen· have been rapid, and that with the new improvements in electrodes and means of measurement of the current, its usage is rapidly resolving itself into a science.

Within the last three years I have been able, by adopting improved electrodes, to increase the maximum strength of current used through a fibroid tumor without causing pain enough to require an anæsthetic, from 25 milliampères to 10 ampères—and this with *one surface electrode*. All who are acquainted with the history of electrolysis in the treatment of fibroid tumors will concede to Dr. Cutter his claim of priority in that field. Respectfully yours,

FRANKLIN H. MARTIN, M.D.

163 State St., Chicago, Feb. 8, 1887.

CASE OF GANGRENE OF THE MOUTH.

Dear Sir:—I desire to report the following case: Stella S., æt. 4 years, was attacked with gangrena orum on December 22, 1886. A gangrenous condition presented itself over all the visible part of the mouth, including the lips. There was fever, restlessness, loss of appetite. The face had a livid, sunken appearance. The discharge from the mouth was fetid and had the ante-mortem odor. The mouth finally presented a brownish-black and dry appearance, with black ulcers on the lips. The mouth and lips often bled. I put the child first on tinc. of chloride of iron and chlorate of potassium, and washed the mouth with a saturated solution of chlorate of potassium. No improvement took place until Christmas day, after I had changed the chlorate of potassium wash for a wash of bromo-chloralum, ℥ij to the pint of water, and to-day, December 31, the mouth is moist and clearing, the fetor is subsiding and the appetite is improving. In fact, the child is convalescing.

About the time I commenced the solution of bromo-chloralum I had a powder of subnitrate of bismuth blown through a quill into the mouth of the child. I think the bromo-chloralum is worthy of trial in these cases of gangrene of the mouth. J. M. BATTEN.

309 Fifth Ave., Pittsburg, Pa., Dec. 31, 1886.

SECRET REMEDIES.

Dear Sir:—In THE JOURNAL of January 15 is a communication from Dr. Amos Sawyer on "Rumex Acetosa" as the probable secret remedy used by traveling cancer doctors for taking out tumors. His falling on the article used *by chance* reminded me of a secret with which I was entrusted some two years since. I had operated on an old gentleman for cat-

aract, and one day, when visiting him, he told me if he received sight from the operation he would give me two valuable recipes: one a sure cure for cancer, the other a never-failing cure for all kinds of sore eyes. To sum up the results of the operation, I got the recipes. Here is the one for cancer: Gather the leaves of the common sheep sorrel, bruise them by pounding, and press out the juice; put the juice in a thin layer in a wide and flat-bottomed new tin pan and set in the sun; when evaporated to the consistency of honey, scrape up with a knife and spread on a piece of cloth large enough to cover the cancer, and bind it on. For two or three hours the pain will be intense, but will then subside. In twelve to twenty-four hours, according to the size and depth of the cancer, it should be taken off, and the cancer will be found loose to its *roots*, and will readily come out. He claimed to have seen it used successfully a number of times, and never knew it to fail. Of course, *diagnosis* was a small matter with him; the *cure* was all he wanted.

On speaking of this one day to one of our old and respected physicians, he told me he saw this same remedy tried twenty-five or thirty years ago. A patient of his had cancer, and friends urged a trial of sheep sorrel. He consented and it was applied. The cancer failed to come away and the patient finally died from it. According to the above formula, would not oxalate of tin be the product? Might it not be well to experiment a little along this line? I write this, also, to show Dr. Sawyer how widespread *secret remedies* sometimes are. Very truly,

T. E. MURRELL, M.D.

111 E. Fifth St., Little Rock, Ark.

ASSOCIATION ITEMS.

AMERICAN MEDICAL ASSOCIATION.

Section of Medical Jurisprudence.

The following valuable and instructive papers will be read at the coming meeting:

James G. Kiernan, M.D., Chicago (two papers): "State Supervision of the Insane;" "Medico-Legal Relations of Epilepsy."

Joseph F. Edwards, A.M., M.D., Editor *The Annals of Hygiene*, Philadelphia, Pa.: "The Suppression of the Illegal Practice of Medicine."

Judge Amos G. Hull, Counselor-at-Law of New York Bar: "Medical Jurisprudence in its Relations to undue influences as affecting Wills and Contracts."

These papers the authors *positively* promise to be *present* and *read*. Besides these, almost positive assurance of papers has been received from Prof. H. O. Marcy, Boston, Mass.; Dr. W. C. Wile, Philadelphia, Pa.; Dr. F. E. Daniels, Editor *Texas Medical Journal*, Austin, Tex.; Dr. E. C. Spitzka, New York; and Dr. Herman J. Boldt, New York.

As titles of papers are received they shall be forwarded for publication in THE JOURNAL.

I. N. QUIMBY, M.D., Chairman.

582 Jersey Ave., Jersey City, N. J.

MISCELLANEOUS.

DR. J. ADAMS ALLEN, President of Rush Medical College, Chicago, was on Wednesday made the recipient of a handsome present from the students of the College. The doctor had entered the class-room for the purpose of delivering his usual lecture when L. E. Tifft, one of the students, interrupted the routine proceedings, and on behalf of his fellow-students presented "Uncle Allen," as the doctor is familiarly called, with a "tripod," consisting of a fat, chubby hand, made of solid gold. The thumb and little finger of the hand were bent so as to touch, the remaining digits being extended. The hand was set on a small block of alabaster, and this, in turn, on a base of black onyx. On the onyx was a solid gold plate bearing this inscription:

UNCLE ALLEN'S TRIPOD.
Remember these three
When we practice the art:
The condition of blood,
The nerves, and the heart.

The quotation is a verse of a college song based on some of Dr. Allen's sayings, and the position of the hand is a favorite one with him when addressing the students. The doctor was greatly surprised on receiving the present, but managed to thank the boys in a way to make them feel happy. He sails Saturday next for Europe, to be gone until September, and this presentation was a token of the good wishes of the students.

THE Governor of New York, in his annual message, has called attention to the practice of adulterating drugs, and denounced it in appropriate terms. Governors of the other States might follow his example with advantage to the profession and the people.

DR. WM. PERRY, the oldest person in Exeter, N. H., and the oldest graduate of Harvard College, died on January 11, aged 98 years. He was the sole survivor of the passengers on Fulton's first steamboat trip down the Hudson River, seventy-nine years ago.

A PHYSICIAN'S PROTECTIVE ASSOCIATION is being organized in Indianapolis.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM JANUARY 29, 1887, TO FEBRUARY 4, 1887.

Capt. Junius L. Powell, Asst. Surgeon, granted leave of absence for two months, to take effect when his services can be spared by his department commander. S. O. 24, A. G. O., Jan. 29, 1887.

First Lieut. Paul Clendenin, Asst. Surgeon, ordered for duty as Post Surgeon at Camp Pena Colorado, Texas. S. O. 14, Dept. Texas, Jan. 26, 1887.

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE HOSPITAL SERVICE FOR THE SIX WEEKS ENDED JANUARY 29, 1887.

Wyman, Walter, Surgeon, granted leave of absence for three days. Jan. 14, 1887.

Wheeler, W. A., P. A. Surgeon, to proceed to Erie, Pa., as Inspector. Jan. 12, 1887.